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MEMOIRS OF THE LITERARY AND
PHILOSOPHICAL SOCIETY
OF MANCHESTER.

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THIRD SERIES.

FIRST VOLUME.

LONDON: H. BAILLIÈRE, 219, REGENT STREET,
AND 290, BROADWAY, NEW YORK.
PARIS: J. BAILLIÈRE, RUE HAUTEFEUILLE.

1862.

Manchester:
Printed by Charles Simms and Co.

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LIST OF ILLUSTRATIONS.

NOTE.

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I. — *On the Origin of Colour, and the Theory of Light.*

By John Smith, M.A., *Perth Academy.*

Read October 4th, 1859.

PART I.

Of every science the elementary laws ought to be profoundly studied and accurately known, but of none more so than the science of light; and yet perhaps there is no physical science, whose laws, as at present interpreted, give less satisfaction to the intelligent student of natural philosophy.

The theory of light as unfolded by Newton is a mathematical description of a physical phenomenon, for the arguments on which it is founded are derived chiefly from the geometrical form taken by that phenomenon; and the wave theory is little else than Newton's theory divested of the emission hypothesis. But to describe a phenomenon geometrically is one thing, to investigate

physical principles is another. Should a physical phenomenon assume a geometrical form, a mathematician may argue upon it and draw from it some valuable inferences without understanding more than the geometry of the phenomenon. The reasoning from the premises may be strictly correct, the deductions mathematically sound, and still the conclusions far from being physically true. The reasoning being founded on geometrical, not on physical facts, only explains such phenomena as are described. Hence the general dissatisfaction with the theory of the different refrangibilities of the rays of light as unfolded by prismatic refraction. The theory resolves itself into the geometrical description of a physical phenomenon, it affords no explanation of the physical process; and as far as the analysis of the physical process is concerned, it is chiefly an attempt to prove that because a ray, after being once refracted, cannot be changed by repeating the same process, it is to be inferred that the ray is reduced to its simple elements; thus assuming, not proving, that the prism is an analytical instrument.

2. To the general student a subject is supposed to be made clear when it is described in mathematical language assisted by geometrical diagrams. But there is nothing in which we are more apt to deceive ourselves, in studying science, than in mistaking the knowledge of the language in which a phenomenon is described for a knowledge of the physical laws, on which the phenomenon depends; and in the study of nature we often deceive ourselves by generalizing without sufficient investigation; for like the student of science the student of nature is apt to misapprehend the language in which nature speaks; he not unfrequently mistakes geometrical laws for physical, and explains the one by the other. He begins to generalize when he discovers that physical laws are in accordance with some of the rules of his previously acquired geo-

metrical knowledge — because, for example, an angle is found to be the measure of force in one instance it is concluded to be the measure of force in others. The laws deducible from the phenomena of the solar spectrum are of this description, and they are certainly very remarkable; but in their application we are constantly compelled to introduce emendations and corrections, to depart from true logical inferences, and to adopt suppositions to satisfy the conditions of other physical problems.

3. I make these observations because I find that however easily ordinary minds are satisfied with the theory of prismatic refraction, all scientific investigators have more or less felt the difficulty of the subject. Neither Sir David Brewster nor Sir John Herschel, as far as I understand them, implicitly acquiesces in the doctrine of Newton, and these are as high authorities on the subject as can be obtained.

The author of the article "Undulations" in the *Penny Cyclopædia* concludes in these words: "Much stress is laid on the accuracy with which the phenomena of diffraction are accounted for on the undulatory hypothesis; but while there yet remains unexplained by that hypothesis so important a circumstance as the different refrangibilities of light, which are satisfactorily accounted for on the corpuscular theory, and while our knowledge of the action of material particles on one another, as well as of the propagation of motion through elastic media is so imperfect, philosophers seem to be fully justified in suspending their judgment concerning the relative merits of the two rival systems."

4. The experiments on light which I am about to explain lead to other conclusions than have hitherto been obtained from the study of the prism. The data are different, and reasoning from the data the conclusions may be expected to be different. It is only by searching for other data

that any improvement or progress can be made in our knowledge of the laws of light; for, judging from the past history of science, human arguments without experimental data would lead us farther and farther from a knowledge of divine laws. Besides, who would rashly encounter, by bare argument, the philosophy of the age; or enter on the study of the prism, without seeking new facts, observations and experiments in defence of a theory opposed to that by means of which so many illustrious investigators explain the facts unfolded by prismatic analysis? I have therefore ventured on another field, and one, I apprehend, much more easily cultivated by the mass of mankind. In the study of light by means of the phenomena of the prism, although it has engaged the attention of thousands of investigators, we are still met at the very threshold by the question, What is a prism? Until this question is answered our progress must be very uncertain. If experiment answered the question it would be all well, but the refraction of the rays of light throws no gleam of sunshine on the internal construction of the instrument;—how then in these circumstances can the prism be considered as an analytical instrument? That it is not such every truly scientific investigator has virtually acknowledged, and still he adopts the inferences derived from its study as the foundation of his inquiries into the physical nature of light. Were the prism an accurate analytical instrument it would leave no doubt on the minds of philosophers as to its indications; few inquirers, however, agree in this respect. Newton saw seven distinct colours, some have seen only four and others three; and even those who agree as to the number of colours in white light differ as to what the fundamental colours are. Of those who allow only three, one party maintains that the three are *Red, Yellow* and *Blue,* another that they are *Red, Green* and *Blue*; a discrepancy which

demonstrates one thing, that there is no distinct analysis by means of the prism. There is thus room left for a new investigation into the cause of colour.

5. In my inquiries into the nature of light I had no fine spun theory to unfold, with its axioms and postulates, by which to demonstrate theorems and solve problems. I was in search of all these. Hence I feel more disposed to describe the mental process by which I was led to undertake the experiments which I have made. I prefer describing the observations and experiments which I made whilst examining the natural phenomena of light, and the arguments which I deduce from them, to formally building up a science. At all events, experiment alone can be accepted as the basis of any new theory, for, in physical researches, no logical or mathematical analysis can take the place of experiment. Has not the great obstacle to the progress of the science of light for these two hundred years past been the building a science mathematically on insufficient experimental data; for if there is one thing more certain than another in regard to the physical nature of light, it is that the data for mathematical calculation are imperfect, and there is nothing which retards the search after truth more than the application of mathematical formulæ to imperfectly ascertained truths. They are then to a great extent assumptions. It may make our researches appear more learned to apply the exact sciences in the illustration of them; but how can the exact sciences be applied to observations not exactly comprehended? I consider, therefore, the exposition of a series of observations a much more scientific and instructive method of arriving at a physical law than any mathematical assumption which, though apparently solving many problems, carefully conceals the physical process and unfolds no method of physical research. Mathematical reasoning is not physical reasoning, and is as easily abused as the

latter, and indeed is so more frequently, when applied to subjects of natural philosophy.

6. To investigate the physical nature of light, to inquire whether the corpuscular or the undulatory theory be true, may by some be considered as a purely speculative or metaphysical discussion resulting in no practical benefit. It matters not, it is said, as far as mathematics are concerned, whether light is an emanation of particles from the luminous body, or the undulations produced by the same body in a highly elastic medium, "for the principal phenomena of light are deducible by strict mathematical reasoning from either supposition." But until mathematics shall be proved to be the only true basis of physical reasoning, such an argument for resting satisfied with what knowledge we have of the nature of light is not at all creditable to science. True philosophy in process of time leads to grand universal laws; false philosophy, or suppositions however ingenious, lead to partial, exceptional and restricted views. Galileo when he was demonstrating by the use of his telescope that the planet Venus had phases similar to those of our moon, and was consequently illuminated by the light of another body and not shining by its own, was doubtless considered by the philosophers of the age as being engaged in a much more sublime investigation, because a more popular one, than when he was demonstrating the law of falling bodies. For little was it thought that by solving the problem of the pendulum he was solving the most comprehensive problem ever solved; that he was teaching to another great investigator the method whereby he was not only to dissipate the mystery of planetary motion but to weigh the matter of the universe itself.

If then, mathematically, it be a matter of indifference what is the true nature of light, it is not so philosophically. What tends to elucidate the true nature of light, may

aid in tracing the true theory of heat, magnetism and the other imponderables.

7. But there are other questions respecting the physical nature of light than the decision of the truth of the corpuscular or undulatory theory. It is equally important to know whether it be a homogeneous or a heterogeneous substance. The grand discovery of comparatively modern times is that of the heterogeneous nature of light; and the facts discovered seem to adapt themselves to, and to be explicable by either theory, for like mathematics, in the opinion of some writers, the compound nature of light is consistent with either theory. But surely it is more than a matter of idle curiosity to decide this question. If light could be proved to be a simple substance, the theory would be rendered more conformable to the recognized simplicity of nature's laws, and would thereby commend itself more to the human intellect, which estimates the divine authority of a law by the universality of its application and its simplicity. The sublimity of the law of gravitation lies in these two qualities — its universality and its simplicity.

8. I have already remarked that, from the discrepancy that exists among philosophers as to the composition of light, there is room for a new investigation into the cause of colour. In the present day however, such an investigation could not be made by means of any refracting medium. Refracting media are already explained, or believed to be explained, on arguments conventionally acquiesced in as established truths, or at all events, as truths which it would be presumptuous in any one in this era of philosophy to question, either by logical or mathematical reasoning.

I propose therefore

I. To give the result of my study of the laws of light without the aid of any refracting substance.

II. To attempt an explanation of prismatic phenomena by means of the laws which have been discovered. And

III. To illustrate my views by experiment.

9. Before entering on the mechanical experiments by which the subject is to be illustrated, I wish to direct attention to two or three phenomena which were the cause of, first of all, creating dissatisfaction in my mind with the received theories of light, and which I consider, when properly understood, will serve as the basis of the true theory. I shall, however, have a difficulty in making myself understood, as I shall be obliged to use a term the adoption of which will not be easily acquiesced in by scientific men as having any physical value until the operation it points out is actually brought under the cognizance of the organs of vision, and even then there may be a difficulty in defining it.

The first of the phenomena referred to is the following.

Experiment I.—Wafer Experiment.

10. A not uncommon experiment is to fix two wafers on a pane of glass or a sheet of paper, about four inches apart, and to look at them with oblique or strained vision until the one is seen to overlie the other, and the two appear to be one.

In making this experiment a great many years ago, I tried it with variously coloured wafers. A red and dark blue, nearly black, gave in a favourable light a green for the single image. A black and a red gave also a green, sometimes a brown and at other times a purple, the difference depending on the nature of the colours and also on the luminous state of the atmosphere.

Having never seen this phase of the phenomenon noticed—and I searched for some notice of it in vain after my attention had been particularly directed to it—I

attempted an explanation, to the best of my ability, according to the received theories of light, but could not satisfy myself.

11. The phase of the experiment to which I wish particularly to direct attention is this: the image of a red wafer being on one eye, the image of a black wafer on the other eye, the resultant of the two, in a favourable light when the one is made to overlie the other, is neither red nor black, but some other colour. Why another colour? Why not red? According to the corpuscular theory black is not an active principle, it is a negation of colour, and should produce no effect on the red. It exacts too much from our faith to believe that two molecules combine somewhere beyond the retina to effect a change of colour before reaching the sensorium. And there is no change in the process of refraction of the red to account for the change of colour or to help us to an explanation.

12. On endeavouring to find an explanation, according to the undulatory theory, I found the same difficulty. Black is not an active principle on the undulatory theory any more than on the corpuscular. If the quality of a colour depends on the length or form of a wave, there is nothing apparently in this experiment to alter the form of the wave, nothing to lessen the number of vibrations, or to reduce its intensity. Why then a change of colour, let the colour be what it may?

13. Trying an explanation of the phenomenon by the theory of complementary colours, there were difficulties equally great to surmount. The retina is not fatigued although the muscles of the eye are,* and there is no reactionary force to account for the colour on the commonly received interpretation of these optical phenomena; so that I failed to find a solution in whatever way I attempted it.

* The eyes may be injured very much in making this experiment.

14. We are apt to think it easy to understand that red and blue become green, or that red and black make a dark dirty green, for we see it often take place; but when we ask ourselves the physical cause of such an effect as that produced by our experiment, it is not so easy to discover one; for according to the commonly received interpretation of the laws of light there is not a sufficient cause for the effect. Both images appear to be seen, but how can the one be seen through the other, or how can they appear transparent, but on the supposition that there are intervals in the movements of the rays from the different objects? We have thus as it were the first step in an argument for the undulatory or rather the vibratory motion of light.

15. In this experiment we have two sensations which differ from each other on the retina, and yet before they reach the sensorium are compounded into another sensation of a different character. Hence the question naturally arises: If the composition of forces or of colours takes place in the organs of vision so simply, does it not presuppose a greater complexity in the laws of light, as commonly taught, than is to be found in any other of the works of God, to affirm that light is a compound heterogeneous substance not only requiring continual decomposition but perpetual recomposition? Is it not introducing into the theory of light ideas as perplexing as the cycles and epicycles which existed in astronomy before the time of Copernicus?

16. After carefully considering the phenomenon in all its bearings, the only satisfactory explanation I could arrive at was, that the two insulated spots, one on the one eye and the other on the other eye, could only be perceived as one object not as two, for the impression at all times received by two eyes of the same spot is single — hence, although we have two eyes, we have virtually or sensationally but one retina — therefore in this case the

change of colour was the effect of the pulsations of light from the red wafer alternating with the sensation of black. But there was a difficulty even here, for the impression of red was not changed when the other eye was completely shut. In answer to this I argued, that there could be no sensation where there was no agitation of the retina. The black wafer could not have been seen where it was, unless the rest of the retina had been agitated with light. In an eye which is shut, every thing is in a state of repose, and there is nothing to interfere with the sensation produced on the other eye, for there is no contending sensation. It is the mixing or uniting of these independent sensations which produces the sensation of change of colour. Black then may be considered as a colour in so far as it is a sensation.

17. If the two retinas are intellectually one, and if each individual retina produces its own impression, it is evident that the number of vibrations on the sensorium must be diminished one half if only one eye is used. The two spots are perceptionally or sensationally one spot, but when considered intellectually or philosophically, the perception is discovered to be a compound of two coördinate sensations, and being coördinate they are sensationally one.

18. But it may be argued that the wave of red light suffered a change before entering the sensorium, for the fibres of the one eye being in motion and the other at rest or quiescent, a diminution of velocity at the junction would be the consequence. This should not affect the length of the impinging wave, which is considered as the cause of colour, if red is an unchangeable element; but even if it did, this supposition would not account for there being no change when the one eye was completely shut, although the same process must still go on.

19. The unavoidable inference from this experiment

then is that colour is produced by intervals of light and no-light. Had there been no intervals in the motion of the incident red ray, it is scarcely possible to conceive that there would have been a change of colour. But a change of colour being perceived, it is but natural to suppose that this change must be caused by the colour or no-colour of the spot against which an object is seen in the intervals of pulsation of the light by which it is made visible. Doubtless, as the number of pulsations of the red was not diminished, and only half of the sensatory nerves were excited, there may be two suppositions; we may either infer from this experiment that the number of vibrations to produce green is only half of those of red, or that only one half of the force is necessary to produce the one colour which it takes to produce the other. Indeed both suppositions may be true. But I am rather inclined to think that this experiment proves that each particular colour does not depend so much on the number of vibrations in a given time, the usual solution, as on the necessity of two independent sensations. For although the colour is changed, the vibrations from the red object are not changed. When I look at a red object the vibrations continue the same, and the impression is not altered; but if I take a red object and make it produce vibrations alternately with a black one, I get the sensation of green, which is exactly what takes place in this experiment, and which I wish to impress, although there is a difficulty in mastering the physical process. There is this difference in the two processes; in the wafer experiment the vibrations of red are not reduced, but when I make red vibrate alternately with black, I reduce the number of vibrations of the red; but the same process goes on, for the red and black, alternating with each other, become virtually two retinæ looking at different objects with exaxlar vision.

20. With this experiment then before me, and these

arguments in support of the explanation of it, it was almost impossible to resist the conviction, that shadow must perform a most important part in the formation of colour, and take its place in every theory of colour.

21. But how is this to be proved?

No one can study light at present without at the same time studying the theory of Newton, and Newton has attempted to prove the very reverse. He has completely abolished shadow from his theory of colour. For, after having reflected, as he says, the various colours of the prism on the "confines of shadow," he concludes "that all colours have themselves indifferently to any confines of shadow, and therefore the differences of these colours from one another do not arise from the different confines of shadow whereby light is variously modified, as has hitherto been the opinion of philosophers." This is quite decided. But so is our experiment. The above experiment tells us, that a force which produces red when red is seen by itself, no longer produces red but green, brown, or some other colour, as the case may be, when the red is seen alternately with black or shadow.* I use shadow in Newton's acceptation of the word, and in that of the ancients; I shall use it in a modified sense by and by.

22. Newton's arguments are drawn from the experiments which he made with the prism. But the phenomena of the prism, being so difficult of explanation and complex, should be compared with other phenomena of a more simple kind, and not with changes perpetually rung upon itself. Such a method of experimenting seems to

* I name these various colours as the effect is not constant but depends much on the state of the atmosphere. And this is not to be wondered at in this instance, where there is a physical difficulty in making the experiment, since Sir John Herschel says, and every one who has experimented can verify what he says, that the image of the sun produced by the prism "varies enormously."

me much like arguing in a circle, always ending where you began, and, without knowing it, starting again on the same track. One of the most elegant writers on physics we have says: "This solar spectrum, as it is called, formed upon the wall, consists, when the light is admitted by a narrow horizontal slit, of four coloured patches corresponding to the slit, and appearing in the order from the bottom of red, green, blue and violet. If the slit be then made a little wider, the patches at their edges overlap each other, and, as a painter would say, produce by the mixture of their elementary colours various new tints. Then the spectrum consists of the seven colours commonly enumerated and seen in the rainbow, viz., red, orange, yellow, green, blue, indigo and violet. Had red, yellow, blue and violet been the four colours obtained in the first experiment, the occurrence of others, viz., of the orange from the mixture of the red and yellow, of the green from the mixture of the yellow and blue, and of the indigo from the mixture of blue and the violet, would have been anticipated. But the true facts of the case not being such proves that they are not yet understood." He adds: "No good explanation has been given of the singular fact of refraction."* Why then constantly appeal to the phenomena of the prism, when so little satisfactory is known about it? The most obvious method of procedure is to inquire in such cases of nature in her ordinary moods, how she performs such and such operations; to inquire how colours are usually produced, and if there are any simple processes which can be explained and received as data for future investigation.

23. Adopting this method of investigation, the first inquiry which suggest itself is,

What is Blue?

* Dr. Arnott's *Elements of Physics.*

How is this colour produced in ordinary cases in nature?

Experiment II., or Card Experiment.

24. If we take a piece of white Bristol board and paint on it with lamp black any figure whatever, and hold it up between us and the light, the figure will appear purple in place of black. If the board is then inclined, so as to allow the side nearest to us to reflect a little more of the light entering the room, the purple will now be changed into a faint blue. This demonstrates, not as Newton said that there is blue in black, when he found that he could obtain blue of a certain order by reflecting black on white, but that black or a shadow and the reflected light *plus* the light transmitted through the card produced the impression of a faint blue. For we have here black in one light appearing as purple, and the same black in an additional light as pale blue.

This experiment is adduced as another argument that shadow has much to do with colour, and as a step in the process of reasoning by which I arrived at what I consider the mechanical demonstration of the cause of colour.

25. I am aware that this and similar cases are considered as anomalous, for which special explanations must be framed; and solely because it has been considered demonstrated that white light is heterogeneous in its nature. For instance, it will be said of the first phase of this experiment that there was a purple in the lamp black, and that the purple only was transmitted, and in the other case that the blue only was reflected, the other rays being absorbed. This is merely, however, ringing a number of changes on words without any positive or experimental facts from which to argue. Such arguments have one advantage; they are easy, and can surmount any difficulty.

26. But in attempting to introduce a new term in explanation of the cause of colour, in attempting to speak of shadows and penumbræ, there are great difficulties attending the subject, for optical science is not familiarized to the use these terms are put to, and besides the negative element is as undefined as the positive; for between the faintest shadow and absolute darkness there are an infinite variety of shades, just as there are between the faintest light and the brightest, if indeed there is a limit to either. But if white light be admitted to be composed of an infinite number of rays crossing each other at all angles — or of one great wave — then the interception of any amount of these rays or of any portion of the wave on any given spot is a shadow. There must be a body to intercept the light, and that body may be either opaque or transparent, for the most transparent body does not transmit all the light which falls upon it. This shadow is generally some degree of grey, unless it be illuminated by a vivid ray at an angle to the ordinary ray, in which case it is coloured. But it is so customary to argue on this subject from the phenomena of the prism, that I shall be told that in all coloured shadows this light had been previously refracted, and the shadow did nothing more than prepare a place on which to reflect it, and prevent its being extinguished by a greater light. Whereas, on the contrary, there is more reason for saying that the number of vibrations being reduced by the interposition of a substance causing a shadow, the light reflected from this shadow alternating with the shadow itself produces a blue, purple, or red effect. In our ignorance of the value of the ray impinging on the shadow, as well as of the value of the shadow (for it is well known that shadows have different values), we have no authority to call them refracted but from a belief in the received theory.

27. Were a ray of light like an infinitely fine wire

reaching from the sun or luminous body to us, and were there rays of every imaginable colour, — or were a ray of light a constant or uninterrupted succession of molecules thrown from the sun or luminous body, which would be the same in effect as the infinitely fine wire,—there would be no need of shadow in a theory of colour, provided there were also a chromatic elective affinity between each terrestrial object and its special ray. But if light is the effect of pulsations repeated at intervals, darkness or shadow must play a most important part in chromatics, as important a part as it does in the natural landscape or in aerial perspective.

28. I therefore feel more disposed to speak in the language of the painter than in that of writers on this subject; for, although the experiments which I have to bring forward demonstrate, as I believe, the vibratory nature of light, I think it will be more intelligible to speak of light and shade, which can be made visible, than of the number of vibrations or form of waves, until these have been demonstrated or exhibited to the sight.

29. Besides, it is almost impossible to believe that there should be a system of rays of different velocities in the luminous ether, or that some of the molecules of ether should be disposed to be differently affected from others. It is possible, however, to conceive, nay, it is almost impossible to resist the conviction, that all natural objects are formed on strict geometrical principles; that some are capable of reflecting more rays and others fewer; that some are more sensitive to light than others, and that the varying degrees of light from different substances produce an equivalent effect on the retina. Intensity is the effect of repeated pulsations on the same physical point. Of course it is possible that different degrees of intensity may produce different lengths of waves on the retina, or within the medium of refraction, and thus cause colour;

but no one has seen these waves or has obtained any proof for their existence. Consequently, it seems to me as useless to talk of the forms of waves of light as of the forms of waves of gravitation, of the forms of the electric, galvanic, or magnetic waves, or of the waves of heat, until something more is known about them. By building a system on hypothesis, we not only bewilder ourselves by making ourselves believe that we are wiser than we are, but we put a drag on science.

30. I fear I may have introduced this part of the subject too soon, by discussing abstract points before I have described the experiments which lead to such discussion. This could scarcely, however, have been avoided, for the idea of length of wave is one which meets us at the very threshold of the science, and will continually present itself under various aspects, called forth by the nature of the subject and the processes it involves, so that it will be very difficult to treat of pulsations without supposing the existence of waves of different lengths. And as my principal object is to substitute a new element in the place of the different refrangibilities of rays, or of the lengths of waves, neither the one nor the other can ever be forgotten or omitted to be put in contrast with the proposed substitute, or the negative element, which is the subject of my inquiries.

31. So far, then, as my argument has gone, neither the emission nor undulatory theory of light, as they are commonly taught, can account for such phenomena as are exhibited in my experiments; it is, therefore, necessary to look for explanations of a much more general character in order to account for such common and every-day appearances.

32. To proceed then with our investigation. The following well known experiment, which has received an explanation generally acquiesced in, will help us in the examination of this important subject.

Experiment III., or Candle Experiment.

Light a candle in daylight, and observe the shadows it casts on the window blind or on a sheet of white paper: they are sometimes blue, at other times purple. The spot on which the shadow is cast, both before and after the candle is lighted, will be, to all appearance, white,— at least the eye can detect no colour; but after the candle is lighted, the spot on which the shadow falls is seen to be coloured. What part does the shadow perform here? Does it merely remove the light of the candle from the spot, and enable us to contrast the light of the sun with the light of the candle? It does more. It shows in this case also that colour is connected with shadow.

33. The received philosophical interpretation of this experiment is to the following effect. I quote from Dr. Young's works: – "If from the light of a candle we take away some of the abundant yellow light, and leave or substitute a portion actually white, the effect is nearly the same as if we took away the yellow light from white, and substituted the indigo which would be left; and we observe, accordingly, that in comparison with the light of a candle the common daylight appears of a purplish hue." Let us look for a moment at this explanation, and see what we can make of it. He says, If we take away some of the abundant yellow light from the light of a candle, and leave or substitute a portion actually white; but he does not tell us how to perform this very nice experiment. True, he puts it in our power to leave or substitute a portion actually white; but this only makes the experiment more mysterious and more difficult to perform. It would appear as if the remarks of Dr. Young presupposed some power of working with a compound beam of light as a painter works with his colours on his pallet. He adds — suppo-

sing this to be done — "that the effect would be the same as if we took away the yellow light from white, and substituted the indigo which would be left." This I certainly will not attempt to deny. I would, however, ask, Did any man ever perform such an experiment on light; or *could* any one perform, or even understand, it? Who ever saw the white light after the yellow was taken from it? or, who knows what the light of a candle is after the abundant yellow is taken from it? Is it the light of the sun, or what? But, according to Dr. Young's theory, there is no yellow to take from white. Even allowing, for the sake of argument, that the yellow — which I suppose is some proportion of red and green, according to Dr. Young's hypothesis — can be taken from the white, are we, therefore, to conclude that indigo would be left? We are thus compelled to make two suppositions, unless we can prove by experiment that such would be the case. It may be fairly asked, Is common daylight made up of only two colours, yellow and indigo? Of course Dr. Young supposes that it is, for his argument here would require it to be so; whereas he says in another part of his work: "We may consider white light as a mixture of red, green and violet — only in the proportion of two parts red, four green and one violet, with respect to the quantity or intensity of sensations produced." If white light is composed of red, green and violet in these or any other proportions, how can we take yellow from it and leave violet? We might be able to compose yellow of certain proportions of these colours; but how we could take yellow from white and leave indigo, on such a supposition, is not easily to be comprehended, unless we make the bold supposition that all the red *plus* all the green *is equal to* yellow, which we are nowhere told is the case, and cannot prove. Yellow and indigo must be by this theory compound, and not primary, colours, and consequently could not be taken or

left by any analytical process known.* Again: it is said, "that common daylight appears in comparison with the light of a candle of a purplish hue." He does not now call common daylight white, but he virtually says that the white light of day is purple compared with the abundant yellow light of a candle. What, then, is white? or, what is the light of a candle?

34. There is no contending against such a method of reasoning as this of Dr. Young's; the premises are constantly shifting for want of intelligible elementary principles. If every new case such as this, in any theory of colour, requires a new hypothesis or some new supposition, to help out an explanation, there is evidently no science, unless conjecture can be dignified with that name. Science requires a general law which shall apply to every case; and in the illustration of that law we must use language which can be understood by the ordinary intellect, or it is not axiomatic; we must appeal to experiments which can be seen to be axiomatic or purely analytical. Without axioms we have no basis on which to build our reasoning; and as the axioms of physical science can only be founded on experiment, to appeal to phenomena which cannot be subjected to experiment, as that of taking away the abundant yellow from the light of a candle, and leaving or substituting white, as Dr. Young has done here, is virtually to abandon philosophy to support a theory.

35. No part of Dr. Young's explanation can be admitted as correct, for even the comparison is not of daylight with the light of a candle. The screen is illuminated with the light of day *plus* the light of the candle, and the shadow is a comparison with the light on the screen, not with the light of the candle. The pitch or tone of the luminous ether is raised, and the eye or retina estimates

* I suspect indigo and violet, and even purple, are often used as synonymes by writers on light.

the united effect of the two lights as a unit. It is only by casting a shadow that the light on the screen is known to be physically compound, and it is by the shadow being coloured that the intellect discovers, as in the wafer experiment, that there must be vibrations or intervals in the motion of both luminous waves or there could be no colour. Besides, this white light must have a certain intensity or the shadow could not be coloured, for as soon as the daylight begins to lessen, the shadow begins to get grey, even although the screen may appear quite white when the candle is extinguished.

36. But this is not all the experiment. Let us light another candle. The united effect on the screen of the daylight and the two candles will still appear white and as originating from one source, but there will be now two shadows of any opaque body; properly speaking, three, for the daylight also produces a shadow; but we are only speaking at present of the shadows caused by the candles. Make the one shadow partly to overlie the other shadow, then the part where the shadows overlie will be of a much deeper tinge of blue than the other parts. This deeper shadow, when estimated by the eye, appears of more than double the intensity of the others. We need not attempt to count beats or pulsations of the ether at present; but supposing the candle to give the greater shadow, let us imagine the effective pulsation of the light of each candle to be double that of daylight. On the screen the number of pulsations would be increased, and consequently also on the retina; and on this supposition there would be five pulsations where before there was only one, and still the eye would estimate the whole five only as one. The pulsations would be one of daylight *plus* four of candlelight, two for each candle. On the darkest shadow there would be one pulsation of light, namely, one of daylight; on the shadows not overlying there would be three pulsations of

light, one of daylight *plus* two of candlelight. Here we have the light of day as compared with that of two candles of a dark purple or blue, and the light of one candle *plus* the light of day compared with that of two candles of a lighter purple, which appears to be hardly in accordance with the principles of logic. For, according to this method of arguing, if we remove the daylight entirely, the light of one candle should appear to the light of two candles as a grey, of which it would be difficult to convince our senses.

37. This is a most instructive experiment, one on which the whole theory of light might be based, had it not already received the form generally acquiesced in. But we shall find that it cannot be explained by means of either theory of light. According to the emission theory there should be no change in the colour of the white, for there has been no change in refraction; every thing externally is just as it was, the daylight not being changed by the light of the candle: and according to the undulatory theory there should be no change in the white light of day, for there has been no change in the wave of light; its intensity has not been diminished nor increased; and the undulatory theory makes no allowance for comparison. One wave may be more intense than another, but it does not on that account, — unless on the doctrine of interference, and interference can only exist under special conditions — thereby diminish the intensity of the less intense; much less can comparison change one wave into another, or, in other words, according to the recognized principles of both theories, decompose the white light into its constituent parts — change the heterogeneous white into the homogeneous blue. If the power of comparison can accomplish so much, why not let it accomplish the whole? why not make it the cause of colour at once? But this is just another instance of departure from or

shifting of logical premises to meet an anomaly, not in the nature of light but in the received theory.

38. This experiment fortifies my argument very strongly, and I have less and less hesitation in saying, in opposition to the received theories, that shadow appears to be as important a principle in any theory of colour as centrifugal force in the theory of gravitation, as important as the term minus in the theory of numbers, as important as the term silence in the theory of music.

Colour of the Sky.

39. It is necessary, however, to search for other natural phenomena in order to verify this conclusion, and among these none stand more prominently forward than the colour of the atmosphere. The colour of the air and sky is explained by every writer on optics, and the common theory is universally admitted to be correct. But it is easier to acquiesce in the fancy of the poet when he sings,

> 'Tis distance lends enchantment to the view
> And robes the mountain in its azure hue,

than in the science of the philosopher. As Newton's opinion is still the opinion of the present day, in place of quoting Newton, I shall quote from *Cosmos* the opinion of Arago, to which Humboldt agrees.

"We cannot explain the diffusion of atmospheric light by the reflection of solar rays on the surface of separation of the strata of different density, of which we suppose the atmosphere to be composed. In fact, if we suppose the sun to be situated in the horizon, the surfaces of separation in the direction of the zenith will be horizontal, and consequently the reflections will also be horizontal, and we shall not be able to see any light in the zenith. On the supposition that such strata exist, no ray would reach us by means of direct reflection. Repeated reflections would

be necessary to produce any effect. In order therefore to explain the phenomenon of *diffused light*, we must suppose the atmosphere to be composed of molecules (of a spherical form for instance), each of which presents an image of the sun somewhat in the same manner as an ordinary glass ball. Pure air is blue, because, according to Newton, the molecules of air have the *necessary thickness to reflect* blue rays. It is therefore natural that the small images of the sun, reflected by the spherical molecules of the atmosphere, should present a bluish tinge; this colour is not, however, pure blue, but white in which blue predominates. When the sky is not perfectly pure and the atmosphere is blended with perceptible vapours, the diffused light is mixed with a large proportion of white. As the moon is yellow, the blue of the air assumes somewhat of a greenish tinge at night, or in other words, becomes blended with yellow."*

40. We are first of all told, on the authority of Newton, that the particles of air are of a size to reflect only blue, and then that the blue is not pure blue, but white in which blue largely predominates. If it be white in which blue largely predominates, the particles of air in any case must be able to reflect other rays than the blue by this admission. Then, again, we are told that the moon is yellow, and that the light of the moon mixing with the blue of the air gives the appearance of green. In this case the air cannot be pure; there must be vapours in it which reflect yellow, or how could there be a mixture to produce green? Or, if the light of the moon is yellow, from whence comes the blue?

41. It is in vain to accumulate instances of inconclusive reasoning in the theory of light, stumbling blocks in the path of science. According to this method of arguing, if the moon is yellow, the air when pure should be black or blue.

* Vol. iii. p. 88. Bohn.

If there are blue rays in the light by which the atmosphere is illuminated, then it should be blue on this theory; if there are no blue rays in the yellow of the moon then the air should be black, as it can only reflect blue rays. But the air has a greenish tinge by night on the admission of Arago. My idea is that this compels him to resort to another supposition, namely, that the vapours of the air must necessarily reflect yellow, and as the air itself reflects only the blue, the blending of the two makes green. Why not at once say that the vapours reflect green, for green is a primary colour on Arago's theory of light. But this is merely an instance, among many which can be adduced, of the inconclusiveness of such reasoning founded on the proportioning of the molecules of air to the molecules of light.

42. I have taken this passage from *Cosmos*, as it is one with which it appears Humboldt was satisfied, to show how difficult it is to account for the colour of the air on any principles of interpretation yet known, for no one who seriously considers the argument can acquiesce in its validity; and I attach more importance to it, as I am convinced that if it can be shown that blue or purple can be produced artificially by merely reducing the motion of a white ray, and by the introduction of darkness or shadow, then the argument for the formation of the other colours must also yield to the same principles of reasoning.

43. There is a similar explanation of the colour of the air by the author of the article "Optics" in the *Library of Useful Knowledge*, which, as it is expressed in very beautiful and clear language, I shall add to the above from *Cosmos*. "We have already seen that the red rays penetrate through the atmosphere, while the blue rays, less able to surmount the resistance which they meet, are reflected or absorbed in their passage. It is to this cause that we must ascribe the blue colour of the sky and the

bright azure which tinges the mountains of the distant landscape." He adds: "As we ascend in the atmosphere the deepness of the blue tinge gradually dies away; and to the aeronaut who has soared above the denser strata, or to the traveller who has ascended the Alps or the Andes, the sky appears of a deep black, while the blue rays find a ready passage through the attenuated strata of the atmosphere." What is to be remarked here is, that the particles or molecules of air in the higher strata of the atmosphere are not of sufficient thickness, to use Arago's expression, to reflect blue. They reflect no colour at all; they appear black. If we adhere to the argument of Arago and Humboldt, supported by the authority of Newton, we are, in this case, compelled to acknowledge that it is the vapours of the atmosphere in the lower strata which reflect blue, and that the air when pure reflects no colour whatever, or that it reflects different colours according to its different degrees of density. On the latter supposition it cannot be the size of molecules which is the cause of colour, but their compactness or the distance of one molecule from another, or, in other words, the ratio of the molecules which reflect light to the spaces which reflect no light; or otherwise the ratio of light to the shade, which is exactly the principle on which I explained the card experiment and the candle experiment; at all events, that the blue is not owing to the size of the molecules of air is evident from the facts supplied by science itself in these examples.

44. There is a respect due to an old and an established doctrine which even criticism ought to venerate, a respect which demands that if we attempt to pull down we shall also endeavour to build up. But the colour of the sky cannot be experimented on in its native element. Our information must be drawn chiefly from accurate observation, assisted of course by such experiments as can be

made available for the purpose. But the scientific explanation, as we have seen, rests completely on Newton's theory of light; so that to object to it may appear to be, virtually, objecting to Newton's theory of large and small molecules, both of light and of matter, of molecules of the thickness requisite for reflecting blue, red, green or any other colour. This, however, I do not mean to attempt, as nothing is really known of the size of molecules of any kind. Lengthened observation has, however, led me to the conclusion that the colour of the sky, and especially "the bright azure which tinges the mountains of the distant landscape," can be accounted for on a more easy and simple principle. I have often remarked that it is not the most distant hills that are the bluest. In a mountainous district the intermediate are of a darker and more beautiful blue than the more distant; and in a pure atmosphere when many clouds are flying, the transparency of the air being great, the nearer hills are of the deepest blue.

45. There are two ways at least in which this may occur. When the nearest clouds are low they not only throw shadows on the hills, but they prevent many of the more perpendicular rays reflected from the hills from reaching the eye. From this circumstance the hills appear in the shade, but the more distant and horizontal rays being reflected from the hills thus placed in the shade, give them the blue colour which is so often described as the blue colour of the air. I have watched until these clouds have passed away, and until I saw the reflected rays grey and brown from rock and heather. In like manner the shadow cast by a single cloud and illuminated by a distant white ray is frequently seen to be blue, and on a hill white with snow the shadow of such a cloud, thus illuminated, becomes very instructive. For we see at the same time on the snow grey shadows and blue shadows; proving that white seen through black does not become blue, but that an insu-

lated white ray, if I may use the expression, reflected from the shadow, produces blue in its intervals of pulsation on the black ground. Painters, as well as other observers, have often remarked that black seen through white is blue. This, however, is not the case, in their acceptation of the term, as the blue and grey shadows on snow prove. At present I am not only arguing for the necessity of introducing into the theory of colour a negative term, namely, shadow or penumbra, but at the same time the equally necessary element, lengthened intervals in the vibratory motion.

46. It is by such observations that we obtain arguments relating to the colour of the sky, as well as arguments for the necessity of introducing a new term into our researches on the cause of colour. From these observations I conclude that blue is produced by a sensation or feeling of no-light in the intervals of pulsation between one wave of light and another.

47. I know that scientific readers will be getting impatient at these arguments, and it will be objected that I am even departing from my own premises; that I am neither proving that colour is formed by reflecting light on the "confines of shadow," as Newton affirmed could not be done, nor am I proving that shadow has anything to do with the formation of colour. For to what do all these arguments tend, if they tend to anything, but to demonstrate the length of a wave—that the darkness I speak of is the hollow of the wave, or the interval between the crest, apex or summit of one wave and another, which apex is the light?

48. That colour cannot be produced by reflecting light on the "confines of shadow" in Newton's acceptation of the term, or in that of the ancients, I admit; but that shadow is necessary to the formation of colour is also true, although in a different acceptation from that of Newton.

Shadow is abstraction of some of the rays of light in any luminous medium. To reflect light on the "confines of shadow" is not to abstract any rays, and consequently is not to alter the nature of the light. To alter the nature of light we must either add to or subtract from its intensity, and, taking the incident light as the unit, colour is produced by abstraction and the co-ordinate sensation of no-light. I have no wish to object to those who prefer to speak in the usual philosophical language about waves and undulations; only I think that when I speak of alternate vibrations of light with shadow, such language is intelligible to the common understanding, while the other, or the language of waves, is not even clear to the understanding of the philosopher. For instance: we cannot refrain from speaking of degrees of cold, although cold is, philosophically, the negation of heat; and, generally, I cannot think it unphilosophical to use as a positive term the name of any feeling or sensation that is cognisable by the senses as being distinct from another, and to which every nation and every language under heaven has given a name. The words which express our feelings or sensations are of themselves the most perfect definitions which can be given of these feelings or sensations; no explanation can add to the accuracy of the conceptions suggested by the words which recall them. Such language I call intelligible, if not axiomatic; it commends itself to every one. Philosophy does not change the sensations — it explains them; and it is of the sensations I am now speaking.

49. But, waiving the philosophy of the language, if it is possible to take any colour and making it vibrate alternately with darkness or shadow, without refraction or without decomposition, convert it into some other colour, then this would be proving not only that light and shadow, or alternate sensations of light and no-light, produce a change

of colour, but it would be doing more: it would be proving the homogeneous nature of light; it would be proving that the luminous ether no more requires three or seven, or any other number of primary colours or waves of different values, to produce all the modifications of light, than the air requires seven or eight constituent principles to produce all the modulations of the gamut. The ancients appear to have thought that shadow or darkness was a positive principle, and so far as it is a sensation it is; and Newton seems to have experimented under that idea, or how could he have thought of reflecting light on shadow? At all events, the ancients considered that they saw some connection between shadow and colour, and Newton set himself to prove that they were in no way connected. It did not occur to Newton, when experimenting with his rays of light (derived from the spectrum) on the "confines of shadow," as he describes it, that he was not in any way altering the constituent elements of his rays of light. He was not reducing their momentum, nor was he making them alternate with shadow. Wherever change of colour is observed in such phenomena as we have been describing, attentive consideration of the whole circumstances will show us that there are two planes necessary to produce the two sensations of light and no-light. In a luminous atmosphere the shadow must be produced by one ray or a portion of a ray, and illuminated by another. In refracting substances, as will be shown by and by, there are two planes at different angles, one reflecting light, the other no-light; for how otherwise could the intervals of motion in the vibrations of the luminous ether be perceived? But although there are, apparently, two different processes in nature, they are virtually one. My experiments are an attempt to produce the same effect by motion, so as to imitate nature in these two processes.

50. What, then, is a shadow? or, what is darkness?

Is it not in this case the interval between the vibration of one wave and another? It is more. It is the sensation of no-light. The interval between one wave and another cannot be seen or appreciated; for there is no sensation, no negative, until the ray is bent from its original direction; and if it is then seen, in its intervals of pulsation, against a basis of a negative character, or against a plane reflecting little or no light, the ray is changed in its character. What is called white light may be changed to any colour. But is not this the existing theory in different words? A moment's reflection will show that it is very different. The present theory admits only positive terms. What I am explaining demands two independent and coordinate sensations in the production of colour.

As every, even the most transparent, body casts a shadow, so there is a probability that even the seemingly most opaque body may, notwithstanding, transmit a certain portion of light. There is, to all appearance, as great a variety of shadow as there is of what is usually called light; and from this point of view shadow may be considered as a positive principle, not from the darkness but from the light which is in it. After a series of experiments we soon find that it is as difficult to discover the value of a shadow as of a ray of light. They have both eluded the most profound researches. We are thus forced to the necessity of viewing shadow as a negative principle, but of different values. In fact we must view it in many cases as a phase of light, although, abstractly, it be perfectly inert. It is this difficulty of not knowing what is light and what is not light that causes such difficulties in our researches into light.* An instance of this is to be found in Newton's

* Dr. Reid sees no propriety at all in the term *passive power:* "It is a powerless power, and a contradiction in terms."

Sir William Hamilton says: "This was one of the most celebrated distinctions in philosophy. . . . Power, therefore, is a word we may use both in an active and passive signification, and in psychology we may apply it

observations on *black*. After much supposition and conjecture, he adds: "And hence may be understood why blacks are usually a little inclined to a bluish colour. For that they are so may be seen by illuminating white paper by light reflected from black substances. For the paper will appear of a bluish white; and the reason is, that black borders on the obscure blue of the first order described in the eighteenth observation, and therefore reflects more rays of that colour than any other." Newton has nowhere, as far as I remember, told us how to perform this experiment; and I confess I cannot understand how it can be done without taking into account other light which must be reflected from the paper.

51. Few people, however, who have made any experiments on light, can have failed to make an observation the converse of that of Newton, namely, that white reflected from black, in favourable circumstances or conditions, is blue. Any one who has examined, for instance, the reflection of a white china plate on a black ground, as black marble or black japan, must have remarked this, for it is a very common phenomenon. The reflection of the white object on the black ground is blue; but no one would infer that a decomposition of the white light has taken place because blue is reflected; it would rather be supposed that, provided the light be sufficiently bright, the portion of it which is reflected from the white china, when insulated from the other portion, produces the observed effect on

both to the active faculties, and to the passive capacities of mind."—Again he says: "There is no pure activity, no pure passivity in nature. All things in the universe are reciprocally in a state of continual action and counteraction. . . . Activity and passivity are not, therefore, in the manifestations of mind distinct independent phenomena. This is a great, though a common, error. They are always conjoined. There is no operation of the mind which is purely active, no affection which is purely passive. In every mental modification, action and passion are the two necessary elements or factors of which it is composed." — *Lectures on Metaphysics*, 16th and 17th.

being again reflected from the black. Nor could any one suppose for a moment that the white contained a blue of some order, and reflected more of that than of any other colour. Of course I am aware that this argument may be turned round in a circle, and I shall be told over again that the black reflects blue, or is an obscure blue of some order! In order to explain phenomena such as this, in accordance with sound logic, we must suppose that the effect is produced by beats or vibrations of light, and that these beats or vibrations of light are connected with darkness or shadow — a proposition which, as I shall presently show, may also be proved experimentally.

52. Colour then, I conceive, cannot be changed "by reflecting it on the confines of shadow," which would not indeed in the least degree reduce its momentum; but if we do reduce the momentum of a ray of light without decomposing it, and by reflecting it at regular intervals with black, that is, giving it by motion, as it were, two different planes — a positive and a negative basis — produce two sensations, alternate indeed, but so rapid as to be considered coördinate; and if we by this means can change one colour into another, then are we warranted in arriving at the conclusion, that the effect of colour is produced by intervals of light and darkness.

53. Although I find it difficult to define the negative term which I am labouring to introduce, but which does nevertheless denote something having a real existence, still it is not more difficult to understand the necessity of it than to understand the necessity of intervals of silence in the science of acoustics. There is one advantage, however, in our experiments; what we cannot define or make intelligible in the present state of scientific language, we can show to exist and to have an appreciable value. By and by, when once the experiments have become familiar, the effects of intervals of light and shade will be as easily

understood as intervals of sound and silence, and they will even throw light on the science of sound.

White Light.

54. I have all along, in compliance with the common, as well as philosophical, language of the day, spoken of white light as if white were the colour of light. But why should light have any particular colour? Is it because white is the most brilliant colour, that we call light white? It is by attributing to light a colour that we are led into inextricable difficulties. Light is ether in motion, and is, we might almost say, no more white than black. Every one who has tried to experiment with light has found it an extremely difficult thing to obtain a white ray. Newton says that the light of the sun is yellow, and he had consequently to modify some of his explanations in accordance with that supposition; and Arago, as mentioned above, has said the same of the light of the moon. From the yellow light of the sun we can obtain red, orange, and the other colours of the spectrum, for we see the effect daily in the spectrum; but how do we, on the supposition that the prism analyses light, get the white which is reflected from the silvered back of a mirror, or the dazzling white of snow? Of course I do not deny that white light really exists, and that it is the brightest light we know; but I do not admit that light is white, or that white light is a compound of various colours, and consequently do not believe that refraction decomposes white light. Every observation which I have made on the nature of light leads me to the conclusion that it is a homogeneous substance and, being a homogeneous substance, can have no colour but what it acquires from the form or chemical character of the objects which reflect it, and which introduce the negative element of which I am speaking. But the discussion of this subject will be resumed presently.

55. So far, then, as our inquiry has gone, we have seen that in every case in which we have attempted to account for one of the primary colours, namely blue, it has resolved itself into light associated with shadow, from which I conclude that light must produce its effects by beats or pulsations on the eye, and that these beats or pulsations must be at appreciable intervals; for if light were an uninterrupted succession of waves at intervals as small as commonly supposed, there would be no use for shadow; shadow could not be seen. It would not produce a new sensation when refracted or reflected.

But it will be asked, can the same principles of reasoning be applied to any other colour? Let us see how they apply to

Green.

56. It is not easy to devise experiments on green which are as readily performed as those described above on blue. The experiment with the red and black wafer is not only a difficult one to make, but it is one which, even when successful, is not satisfactory to many minds.

57. There is another experiment of the same kind which, although satisfactory to myself, has the same disadvantage of being difficult to make, and it requires moreover strong sunshine. I have observed, when waving a red silk handkerchief in the air, in strong sunshine, that the edges appeared of a dark green. Although I have made this experiment in the presence of young people with good eyes, and have at the same time told them what to observe, still I cannot say that I have ever got any one who really saw what I directed him to look at. But this experiment proves, when it is successful, that green or change of colour is produced by red light and no-light. Beyond the red, no light is reflected to the eye, and the bright red alternating with darkness produces the sensation of green. Acciden-

ERRATUM

Page 37, line 8 from bottom, *for* "reflected" *read* "transmitted," for according to hypothesis nothing but transmitted light could enter the diving bell.

tally I had another opportunity of observing this phenomenon on a larger scale, at which two people were present besides myself. In a court at a little distance two persons were beating a red carpet; the sun was shining at the time. When the one struck it there was a large wave of red; when the other struck it there was a large wave of green. I examined the appearance very minutely, and then desired those who were standing by me to observe, and tell me what they saw. One said he saw nothing remarkable, only the red carpet; the other that he saw red and green alternately. These observations being rather difficult to repeat, I merely adduce them as additional arguments in favour of the theory which I am attempting to establish, and perhaps they may be of use to others also.

58. There is one experiment, however, which is universally admitted to have been performed with extreme accuracy by one of the greatest philosophers of his age, and which may be made available for our purpose. I refer to Halley's celebrated diving-bell experiment. His attention, when under water, was directed to the colour of his hand, and to that of the water below. The upper part of his hand appeared red, the water and the lower part of the hand green. Newton's explanation of this is, that water transmits the red, and reflects the green rays.

It is difficult to understand this explanation, for to me it appears that the light from both the upper and lower part of the hand was reflected light. The upper part of the hand reflected more of the subdued reddish light with which he was surrounded; the lower less, for it was in the shade. In consequence of the depth of the water below, few of the rays entering the water were reflected thence, and the faint pulsation of reddish light alternating with the darkness below, produced green, in a manner similar to that in which we have seen the colour of the sky is

produced. Even in this experiment, then, one of the most accurate experiments on record, we see that the colour green is associated with shadow, and is caused by intervals of red and darkness, a conclusion which may be arrived at by other experiments without subjecting the light to refraction, as it is in this instance.

59. There is another observation strongly corroborative of this conclusion; but since the effect, just as in the preceding instance, might be supposed to be due to refraction, I have great reluctance in bringing it forward at this stage of the investigation, but I found it of great assistance in my inquiries into the cause of colour. Often on a frosty night I have remarked on shop windows the light within surrounded with a halo, similar to that which is frequently seen around the moon, and of a similar colour. Between the image of the light and the halo there was a dark ring, which was in many cases quite black, but in others a very distinct green of various hues. The question arises, What is the cause of this green colour? Considerable difficulties attended the explanation of the phenomenon. For a long time I could discover no explanation, and to resort to hypothesis was contrary to the rule which I had laid down for myself. I resolved to wait patiently, and observe as carefully as possible, until nature told her own tale. On riding rapidly along, these appearances struck me more frequently. On such occasions I constantly observed either the green or the black ring. After continued observation I became convinced that the green was only perceived when another light than that causing the halo was reflected from the window. This I saw was constantly the case when there was a light opposite the one creating the halo. Now this fact led to the same inference as that to which I was conducted by my other observations, viz., that light proceeds by pulsations, and that colour is connected with darkness or shadow, with

intervals of light and no-light. I could not adopt Newton's hypothesis in Halley's experiment in the diving-bell, and say that the glass reflected the green and transmitted every other colour, for that was not the case. It could only be explained on the same principle by which it has been attempted to account for the blue shadow cast by a lighted candle on the window-blind in daylight. In the phenomena just described, refracting substances, water and glass, play a part; and they afforded only arguments of a secondary kind, since Newton had forestalled the explanation by his theory of refraction, reflection, and transmission.

60. It will be remarked that green is always associated with a red or a yellow colour in these observations. I accordingly attempted to produce green by the mere motion of red light alternating with black, and I think successfully. The green obtained by the motion of the red handkerchief was of this description, but it was not of so decided a nature as I could wish, as I could not make it visible to every one. I do not, however, consider that light should be at first red in order to produce green; I only mean to assert that a ray of the value of red, when made to vibrate alternately with black or shadow, produces green in a clear atmosphere. Another argument bringing us nearer and nearer to the homogeneous nature of light.

61. I willingly grant, as I formerly remarked, that the combination of two such sensations as red and black, or of yellow and black, producing green, is not apt to strike one as at all remarkable, as we often see the effect produced; but the fact of its being common does not make the investigation of the cause the less necessary. It must be remarked that the mere reduction of the intensity of a ray will not change its colour.

Inquiry into the cause of Red.

62. Had we begun to inquire first of all into the nature of red, our researches would have made slower progress even than they have made. It was by observing shadow to be always so closely connected with the colours blue and green that it became necessary to assume that colour was produced by intervals of light and no-light, vibrating at greater intervals than science teaches. It is difficult to see shadow in the usual acceptation of the term in connection with red. Whether we look at the ever varying and gorgeous phenomena of the atmosphere, or at the phenomena produced by refracting media, there is little indication of shadow to be seen. At the setting of the sun many of the rays of the lower limb of the sun are intercepted by the earth, and by the density of the atmosphere; and those rays which do reach us direct are reduced to parallelism; there is little crossing in comparison with the light of noon. Of course, since fewer rays reach the eye there will be fewer vibrations, less intensity of light; but this of itself cannot produce colour unless there is also a negative element present, and as this negative element is not discoverable, there is nothing to point out to the intellect that there must be vibrations or pulsations. It might be an uninterrupted flow of light of a different physical character from the ordinary daylight; in fact it might be a homogeneous light compared to the light of day, and might be produced by refraction, or by the absorption of other rays differently constituted from it.

63. In a fog at noon the sun is often seen of a deep red, with its intensity of light so reduced as to be looked at without inconvenience. I have often remarked on these occasions that it appeared as if one were looking along a red tunnel. The rays were evidently not crossing each

other much. These two phenomena, the setting sun and the sun seen through a fog, may be adduced as leading to the conclusion, that red is produced by motion in comparatively straight lines, without any intersection of rays. I think that this conclusion is supported by Halley's diving-bell experiment.

But the question, What indication is there of a negative element? still remains unanswered.

64. Were I to adduce arguments founded on observations on the eye, I should be met by the objection, that the eye is not a perfectly achromatic instrument — an assertion, however, which no one can believe unless he also believes in the heterogeneous nature of light; since certain phenomena, connected with the eye, cannot be explained on any other supposition by those who adopt that theory. My eye, I know, is not now perfectly achromatic, but I remember the time when it was so; and I believe every good eye to be achromatic. But letting this pass, every one who has experimented on light knows that he can convert his eye into a prism, so as to produce all the coloured dispersion which is seen in the prism. If we look at a candle over the edge of anything — the finger will do as well as any thing else — and cover somewhat more than one half of the pupil of the eye, yet so as to see the candle distinctly, thus intercepting a multitude of the rays, but allowing those which do enter the eye to enter in one direction only, the lower part of the candle will appear red, the middle yellow, and beyond there will be a line of blue. Of course this order depends on what part of the pupil is covered. It is evident that the whole of this red or yellow is not produced by refraction, allowing that the refractive power of the eye is the sole cause of the colour. It is impossible to omit observing that the dispersion of light on the penumbra caused by the object has more to do with the colour than refraction. A minute

inspection of this phenomenon will show us that there is a penumbra cast by the object on the eye from the substance interposed between the light and the eye, and that it is this penumbra which is coloured. Is not the red of the atmosphere produced in the same manner? Is it not the light of the sun, reflected from the atmosphere at intervals with the penumbra of the earth cast on the same atmosphere? In the one case we can detect the penumbra; in the other we can only infer its existence. But the inference is perfectly legitimate.

65. I may be told that to produce this parallelism in rays, refraction is necessary. True, refraction is necessary; but not for the purpose of decomposing a compound heterogeneous substance into its constituents parts, but rather for the purpose of compounding an active with an inert or passive sensation.

66. But it will be asked, Is not this what the wave theory teaches? If it is, it teaches it in a very different way. The wave theory assumes different lengths of waves; and these different lengths must be owing to the compound nature of light, or to some affections in matter not understood, and only hypothetically assumed.

67. These observations on red are satisfactory only so far as the phenomena are seen in connection with shadow; but combining them with others, they may assist in establishing the elements of the science of light; and it has been considered necessary to refer to them, because the proofs derived from experiment would not be understood nor acquiesced in, unless accompanied by preliminary remarks illustrative of the method of investigation, and showing that the experiments were not made accidentally or at random, but for the purpose of developing the laws on which the optical phenomena everywhere surrounding us depend.

Having thus gone over the arguments on which the theory

of light, which I am endeavouring to establish, rests—at least so far as they relate to the primary colours, blue, green and red — I shall for the purpose of recapitulation describe two other experiments which can be made at any time, and which, to my mind, are beautifully illustrative of the whole theory.

Observation, A.

In a room with a good bright fire, the flame of which is so strong as to produce a distinct shadow on the ceiling of any object such as the gaselier, suppose we light a candle or a jet of gas, then the candle or gas will also form a shadow of the same object, but the two shadows will be coloured, the one differently from the other. The shadow formed by the jet of gas will be brownish-red, for the flame of the fire which illuminates it is red; the shadow cast by the fire will be blue, for the light of the gas which illuminates it is white or nearly white.

Now if we withdraw one of the lights, say the jet of gas which is easily managed, the ceiling will still be white, as far as the eye can judge, but the shadow is grey because it is no longer illuminated with the gas-light; and the same thing happens to the shadow from the gas, it also becomes grey when the light of the fire gets dull: thus demonstrating, not that the one light compared to the other is as brown to blue, as we were told in the experiment with the candle and daylight by Dr. Young, but that each coloured shadow is some fraction of the unit of force agitating the retina; but that fraction of the unit of force agitating the retina cannot be calculated until we know the value of the unit and the value of each shadow. For that each of the shadows has a value, independently of the illuminating power exerted on it by the other's light, may be seen by examining each of them separately. It will be observed that neither of them is absolute darkness, or a complete negation of force.

68. From this and the foregoing observations I conclude, that the value of a colour is known only when both the active and the passive or negative elements are known, and that there is no colour without the negative as well as the passive element. They are coördinate elements of colour.

A similar observation beautifully illustrative of the above may be made almost every day when the sun is shining. I see every morning on my window blinds, when the weather is favourable, coloured shadows such as I have been describing. Let us subject them to examination.

Observation, B.

Before a sheet of white paper used as a screen, hold a small opaque body, as a pencil, so that its shadow may fall on the paper. There will be seen two shadows; one blue or purple, depending on the state of the atmosphere, or at times on the distance of the object which casts the shadow on the screen — the other, red, ochre or yellow. These are the same colours which were seen in the candle observation or experiment. There is evidently here a partial analysis of the illuminating medium, the separation of it into two distinct waves. But the question arises, Are these two distinct waves red and blue? I say they are not. But this is a problem not so easily solved. We must, therefore, resort to our former observations for assistance, if they can afford us any.

The state of the problem is this. We have two shadows; one, blue or purple — the other, red, ochre or yellow. Whence come these colours? At the time these shadows are seen, some of the rays from the lower part of the sun are still intercepted; there is, consequently, a great penumbra in the direction of the ray, although not easily detected but by such an experiment. The rays within this penumbra are direct; the rays without it are reflected,

as is seen by the direction of the shadow. The shadow produced by the rays within the penumbra is illuminated by the light without the penumbra, and it is, consequently, blue or purple. The shadow cast by the light without the penumbra is illuminated by the stronger and more direct light which is within it, and is red or yellow. This is precisely similar to the fire and candle experiment; but the origin of the two rays is not so easily discovered. Terrestrial reflection has often much to do with the colour of the red ray.

I have no doubt that these remarks will be met by the counter assertion, that the light of the atmosphere is blue, and that the other, or the red light, is refracted light, red and blue united making white.

In the present state of the science this is quite a legitimate objection. Let us, however, test the truth of the usual explanation by the following

Observation, C.

Suppose we ask the question, Do the two rays, red and blue, which are obtained in the above experiment, make white? If they do, if we unite them again, the resultant should be white. Let us then unite them. It is done as follows.

In place of one pencil, or opaque body, hold up two. There will now be four shadows, each object producing two. Let us then cause the red shadow of the one to overlie the blue of the other. These two shadows should by uniting produce white; but it will be found that the compound shadow is not white, it is green.

But why has white not been produced by the combination of these two shadows in the manner described? Because, although the two lights have been united, they are each compounded with shadow. There are two waves of light and two shadows, and they are each, as it were, insulated

from or sections of the great photospheric or ethereal wave. We thus see the value of the negative element when once it has been introduced, and the difficulty, the impossibility of again separating it in any colorific ray. I therefore hold to my argument, that light and shadow are coördinate elements of colour.

But perhaps I may be told, that if red and blue do not make white, then red, blue and green, the colours which we have now got, make white. Dr. Young says they do. And if by combining them I could prove that I produced purple, I might then be told that red, green, blue and purple make white; and so on *ad infinitum.*

From this experiment, combined with the former, a good resumé of my whole argument may be obtained. For although in the experiment with the two wafers we could not but infer that green was a combination of two sensations, it did not show us that shadow took any part in the formation of colour; it merely enabled us to draw the inference, that the change of colour could not be accounted for but on the supposition that there is a vibratory motion in light. These experiments, I think, give us solid arguments to conclude that light and shadow are coördinate elements of colour.

PART II.

EXPERIMENTAL.

69. In my researches into the cause of colour I was led — in order to satisfy the conditions of several problems — to introduce in considering the subject a negative term, not in accordance with the commonly received theories. Newton, in his theory of light, recognises only positive terms, for he considered that he had demonstrated

the existence of rays of light in the sun for every colour in the solar spectrum or even in nature. He concludes, prop. 2, bk. i. pt. ii. of his *Optics*, in these words: "From all which it is manifest that if the sun's light consisted of but one sort of rays, there would be but one colour in the whole world, nor would it be possible to produce any new colour by reflections and refractions, and by consequence that the variety of colour depends upon the composition of light." To which he appends the following definition:

"The homogeneal light and rays which appear red, or rather make objects appear so, I call rubrific or red-making; those which make objects appear yellow, green, blue and violet, I call green-making, yellow-making, blue-making, violet-making, and so of the rest. And if at any time I speak of light and rays as coloured or indued with colours, I would be understood to speak not philosophically and properly, but grossly, and according to such conceptions as vulgar people, in seeing all these experiments, would be apt to frame. For the rays, to speak properly, are not coloured. In them there is nothing else than a certain power and disposition to stir up a sensation of this or that colour. For as sound in a bell, or musical string, or other sounding body, is nothing but a trembling motion, and in the air nothing but that motion propagated from the object, and in the sensorium it is a sense of that motion under the form of sound; so colours in the object are nothing but a disposition to reflect this or that sort of rays more copiously than the rest. In the rays they are nothing but their dispositions to propagate this or that motion into the sensorium, and in the sensorium they are the sensations of those motions under the form of colours."

In his letter to the Royal Society he says: "Nor are there only rays proper and particular to the more eminent colours, but even to all their intermediate gradations." In

another part of the same letter he says: "These things being so, it can be no longer disputed whether there be colours in the dark, nor whether they be qualities of the objects we see; no, nor perhaps whether light be a body. For since colours are the qualities of light, having its rays for their entire and immediate subject, how can we think those rays qualities also unless one quality may be the subject of and sustain another; which in effect is to call it substance. We should not know bodies for substances were it not for their sensible qualities; and the principal of those being now found due to something else, we have as good reason to believe that to be a substance also.

"Besides who ever thought any quality to be a heterogeneous aggregate, such as light is discovered to be. But to determine more absolutely what light is, after what manner refracted, and by what modes or actions it produces in our minds the phantasms of colours, is not so easy. And I shall not mingle conjectures with certainties."

70. The experiments which I am about to describe appear to me to establish a theory the very opposite of Newton's. They prove that the sun's light consists of but one sort of rays, and that there are no such rays as red-making, green-making, or violet-making rays.

71. But it may be said that the undulatory theory of light is directly opposed to that of Newton. In point of fact, however, there is little difference between the corpuscular theory, as explained by Newton, and the undulatory, except in the more rational supposition by the advocates of the latter of an ambient ether to convey the sensations of colour, just as the air conveys the sensations of sound. For the undulatory theory adopts Newton's view of the composition of light, or, what is the same thing, supposes that there are waves of different lengths either inherent in the nature of light itself or produced by the nature of the refracting

media, for there seems to be some uncertainty on the subject in the scientific mind. One scientific writer says:* "From the foregoing we conclude, then, that the peculiar colour and refrangibility belonging to each kind of homogeneous light are permanent and original affections not generated by the changes which light undergoes in refractions or reflections; and therefore that these properties are inherent in the rays previous to their separation by experiments."

Another says: "The dispersion of light into the colours of the spectrum, consequent on its refraction through a prism, is explained by supposing that transparent bodies attract different sorts of light unequally; their refraction is therefore unequal; and thus, on their emergence from the refracting medium, the rays become dispersed. . . . . According to the undulatory theory the production of coloured light is supposed to be owing to the different velocities with which the particles of ether vibrate; and thus a distinct sensation is excited in the eye analogous to that which is experienced by the ear from the different velocities with which the undulations of the atmosphere are propagated. As high notes and low notes result from the different velocities of these aerial undulations, so the different colours owe their origin to the unequal velocities with which the ether vibrates . . . . The different refrangibility of light, and the prismatic dispersion consequent upon it, admit of being very easily explained; for a wave that vibrates with a higher velocity than another will suffer a different refraction from that suffered by the latter, if the two waves be transmitted through the same medium, as *ex. gr.* through glass."†

These arguments presuppose that it has been demonstrated that different velocities exist inherently in the

* *Treatise on Light and Vision*, by the Rev. H. Lloyd, p. 198.

† Peschel's *Elements of Physics*, pp. 90, 91.

different rays of light, but the writer just quoted seems also to suppose that matter may modify them, for he adds: "Lastly, as regards the colours generated by reflection of light, it may be seen how it is possible that the velocity of the incident wave may sustain a different modification from different bodies, in consequence of which modification of velocity different substances will be of different colours."*

72. One of the most acute and scientific writers of the present day, a star of the first magnitude in the scientific galaxy, when speaking of this subject says: "It will be shown presently that the deviation of light by refraction is a consequence of the difference of its velocities within and without the refracting medium; and when these velocities are given the amount of refraction is also given. Hence it would appear to follow unavoidably that rays of all colours must be in all cases equally refracted; and that therefore there could exist no such phenomenon as dispersion. Dr. Young has attempted to gloss over this difficulty by calling to his aid the vibrations of the ponderable matter itself, as modifying the velocity of the ethereal undulations within it, and that differently according to their frequency; and thus producing a difference in the velocity of propagation of the different colours; but to us it appears with more ingenuity than success."† I may remark that even this supposition of Dr. Young's is not an original hypothesis, for Newton, in his 17th question at the close of his *Optics*, says: "If a stone be thrown into stagnating water, the waves excited thereby continue some time to arise in the place where the stone fell into the water, and are propagated from thence in concentric circles upon the surface of the water to great distances. And the vibrations or tremors excited in the air by percussion continue a little

* Peschel's *Elements of Physics*, p. 92.

† *Ency. Metropol.* Article, "Light," p. 449.

time to move from the place of percussion in concentric spheres to great distances. And in like manner, when a ray of light falls upon the surface of any pellucid body, and is there refracted or reflected, may not waves of vibrations or tremors be thereby excited in the refracting or reflecting medium at the point of incidence, and continue to arise there, and to be propagated from thence as long as they continue to arise, and be propagated when they are excited in the bottom of the eye by the pressure or motion of the finger, or by the light which comes from the coal of fire in the experiments above mentioned? And are not these vibrations propagated from the point of incidence to great distances? And do they not overtake the rays of light, and by overtaking them successively do they not put them into fits of easy reflexion and easy transmission described above? For if the rays endeavour to recede from the densest part of the vibration they may be alternately accelerated and retarded by the vibrations overtaking them"

A moment's reflection will show us that both of these suppositions cannot be admitted into the same theory of colour without rendering it extremely complex, not to say incomprehensible.

73. Sir John Herschel sums up a review of the two theories in these words: "The fact is that neither the corpuscular nor the undulatory, nor any other system which has yet been devised will furnish that complete and satisfactory explanation of all the phenomena of light which is desirable. Certain admissions must be made at every step as to modes of mechanical action, where we are in total ignorance of the acting forces; and we are called on where reasoning fails us occasionally for an exercise of faith."

74. I have adduced these authorities to show that with the admissions and corrections and exercises of faith which

we are called on to make, the two systems do not in reality differ so widely as might be supposed, — so very little, indeed, that I am warranted in thinking that the arguments which will overturn the emission theory will be apt to sap the foundations of the wave theory; for I do not consider the latter theory to consist in the mere hypothesis of a circumambient ether, but rather in the principles of interpretation which it adopts in the explanation of natural phenomena. For that the two systems are intimately connected is manifest when we consider that not only has the theory of the composition of light been adopted from Newton, but that it was Newton who first of all ventured on the calculation of the length of wave proper to each element of the compound. Neither theory can indeed altogether dispense with the assumption of the compound nature of light. Newton's theory presupposes a difference in the size of the molecules, and consequently a difference in the length of the wave of each molecule, so as to account for the difference in the refrangibility of the differently coloured rays; the undulatory theory takes for granted either an inherent difference in the original rays of the light itself, and considers the ether therefore to be a compound substance; or it requires a difference in the velocity of the ether within and without the refracting media, supposing the light to be homogeneous; which, however, leaves the phenomenon of dispersion unaccounted for, and consequently the advocates of the wave system are compelled to fall back on the theory of the compound nature of light as unfolded by Newton.

75. Both theories recognise only positive terms; neither admits a negative element, or any inert property in matter in order to account for these phenomena. But one of the strongest arguments in favour of a negative term on the wave hypothesis is founded on the nature of the element itself. I do not mean to discuss the many difficult ques-

tions raised by the opposing theories, my intention being to keep one point, and one only, in view; viz. to show by reasoning and experiment the necessity of introducing a negative term in the explanation of the phenomena of colour, and to follow out the conclusions to be derived from the experiments. It may therefore be advisable to state the argument in favour of a negative term derivable from the ether itself before entering on the experimental part of my subject.

76. It has been customary to investigate the theory of light by the analogy of sound, sound being formed in an ambient fluid analogous to the supposed luminous ether; that is, both being produced by a similar physical process, sound and light are supposed to follow the same laws. They may obey the same laws; this I am not disputing; but these laws must be investigated on different principles. For allowing that they do obey the same physical laws, still at the commencement these laws operate in a different manner. It would, therefore, not be philosophical to assume a complete analogy. For in the theory of light, motion or an undulating ether is given us; we have therefore to investigate the various laws by which motion is diminished or modified so as to produce variety of colour. In the theory of sound, silence or rest is given us; we have therefore to investigate the laws by which motion is produced in a circumambient fluid so as to give rise to variety of sound or tone. The one deals with an analytical process, the other with a synthetical process. One process consists in the resolution of force, the other in the generation of force. Sound is created by a solid body communicating its vibrations to the surrounding atmosphere; colour is produced by a solid body intercepting or reducing the vibrations of a vibrating ether. The one is consequently the inverse process of the other, and it should not be assumed that they follow the same laws

because they both result from the vibrations of a circumambient ether, until these laws are investigated. Assuming, however, the analogy between sound and light, and arguing on the assumption, it follows that the theory of light requires a negative principle to carry out the analogy. Sound requires an active element to put inert matter in motion; colour requires an inert or opposing element to modify the motion of an active agent.

77. When we know the length of a string, which when struck under certain conditions produces the key note in music, we can estimate the lengths of the other strings which will produce the notes in the entire scale. These have been studied for many ages, and can be very accurately calculated, although the calculation is still subject to a rectification, determined by trial or experiment. Could we estimate the value of a shadow, and did we know the value of the illuminating ether, the same thing might be done with light.

78. Every known substance reflects light, whether it be opaque or transparent. It could not be seen otherwise. That portion of the light which is reflected must cast a shadow, and where can that shadow fall but in the first place within the transparent substance? and if the substance is a refracting substance a portion of the shadow will appear coloured under certain conditions as will be shown by and by. As the distance from the refracting substance increases the light decreases, while the shadow or penumbra which has been cast by the reflected light increases; they are thus coördinate elements, as will be presently shown, in the formation of colour.

79. But darkness, or the effect of shadow, may be produced by reflection as well as by the interposition of a solid body. It is chiefly by reflection that darkness is produced in nature, at least such darkness as I am speaking of. The geometrician will have no difficulty in conceiving

how this can be done. Transparent or partially transparent bodies composed of different plates or laminæ not parallel divide the incident light into different sections; some reflecting light to the spectator — others from him, or, which is the same thing, producing the effect of darkness.

80. The intensity of these shadows may be conceived as being analogous to the lengths and tensions of musical strings; and I have little doubt they are so: but I have no data on which to found a precise analogy, and all speculation is foreign to my plan. Even were I to resort to the analogy of sound, there are other difficulties than those mentioned; and not the least is the difference of the instruments by which we see and hear. I expect, however, that the discoveries which have been made in light will lead to new methods of research in several other sciences; and consequently I have no greater desire to follow out the analogy with sound than to apply these discoveries to other sciences. As, however, a comparison has frequently been instituted between the two phenomena of light and sound, I wish to show that if the analogy is to hold good, a negative term must also be introduced into the science of colour.

81. None of the theories of light deny to it motion. We may, therefore, assume that light is ether in motion, or the agitation of a circumambient ether.

82. Where there is reflection there is light; where there is no reflection there is darkness. The most convenient substances for representing these two different states are white paper and black velvet, or lamp-black used as a pigment. It is with these substances that my experiments were made: but a bit of Bristol board having various figures painted black on it, or cut out of it, I found to serve equally well; for, there being no reflection from the spaces cut out, they appear black.

83. By means of the experiments to be presently described, I think that I shall succeed in establishing the following propositions:

I. That colour can be produced by the motion of a beam of white light without the aid of refraction.

II. That to produce colour it is necessary to produce a shadow.

III. That a ray of light of the same intensity, repeated at different intervals, gives rise to different colours.

IV. That, taking the intensity of the luminous ether at a given time as the unit, each colour consists in some ratio of the luminous to the non-luminous element, however obtained, and is not a constituent principle of the luminous element itself — that it is not the fraction of an active element, but the ratio of an active to a passive element; or conversely,

V. That by reducing or increasing the velocity of the machine, or, as we may call it, the ray, and preserving the same intervals of light and shade on the diagram, the same effects will be produced as if the ratio between the light and shade had been altered, or as if the light were reflected from a new substance.

VI. That the refrangibility or reflexibility of a ray does not indicate the colour or quality of a ray.

VII. That light, or the luminous ether, is consequently a simple uncompounded substance.

VIII. That colour is a compound sensation, or the effect of two coördinate sensations — an active and a passive, a positive and a negative — of motion and no-motion.

84. Having already, without previously developing any formal theory, exhibited the process of reasoning which compelled me to refuse my assent to the commonly received theory of light, I will proceed to describe the experiments, which served to demonstrate the above propositions, just as they were made, and rude and elementary though they sometimes were. The theory will develop itself out of the experiments, and the above propositions will be proved by the sum of the evidence adduced, although possibly not in the order announced.

85. The first thing then to be proved is, that colour can be produced without the aid of refraction.

The wafer experiment, the card experiment and the candle experiment were so very interesting and, according to the view which I had taken of them, so very instructive, that I was resolved to make them the basis of my new experiments — that is, to produce, if possible, the same effects mechanically. But seeing that the whole optical science of the day was so much opposed to my idea, I naturally felt some reluctance in attempting to verify it by experiment. As, however, neither the emission theory, nor the wave theory, nor the theory of complementary colours, could account for the effects produced, I became disposed to yield to my own convictions, and the more so, as I could see no defect in my process of reasoning, after having subjected it over and over again to the most rigid scrutiny I could apply to it.

86. Accepting it as a well established fact, that light is not perceived until it falls upon and is reflected from some material substance, I concluded that if a beam of white light could be made to move rapidly over a space which reflected little or no light, I should be able to change the colour of the light; for, by thus reducing the number of vibrations, I considered that the effect would be analogous to that produced by a refracting substance. At all events,

if light and shade should really produce the effects anticipated, even though the experiment should not turn out so satisfactory as to enable me to exhibit any colour I could wish, still, should it even change the tint in the slightest degree, the fact would afford some evidence in favour of my argument.

87. In want of something better I first of all made a piece of pasteboard into a disc, and coloured the whole black, except a narrow ray radiating from the centre to the circumference, which portion was left white. (*See plate IV. fig.* 1.)

I then made the disc revolve on a pivot. The experiment was not very encouraging, but it was so far successful that, just as the motion was about to stop, or when it was beginning to get slow, I perceived, as I thought, a tinge of brown. I directed the attention of several young persons with good eyes to the appearance, but they did not perceive anything. I was not, however, discouraged by this, as I was aware that it requires some experience of a special kind to observe accurately. From repeating the experiment very often I was more and more convinced of the correctness of my own observations; and I also arrived at the conviction that, *if colour was to be obtained by motion, it must be by having a slower and more regular motion than I could command with such an instrument.*

This, of course, was merely an introductory experiment previously to getting an apparatus which would give a greater command over the ray of light with which it was intended to operate.

88. The apparatus which I ordered was of the following construction. (*See plate IV. fig.* 2.) I call it

The Chromascope.

It consists, as will be seen, of one wheel driving another wheel, so as to obtain multiple and steady motion. For

every revolution of the larger wheel the smaller makes eight revolutions. On the pivot of the axle of the smaller wheel there is a small chuck or holdfast, by which the disc or figure, or the substance reflecting the light with which it is intended to operate, is fixed to the machine; this was always a small piece of card of various forms. I tried thin plates of silver, but they did not suit the purpose so well as the bits of Bristol board. I expect, however, all these experiments to be improved by means of more perfect apparatus, as I have hitherto not been able to devote as much attention to them as I could wish, my time being too much and too laboriously occupied in a most arduous profession. Indeed the whole of the experiments were first of all made under a jet of gaslight, after eight o'clock in the evening, and it was sometimes months before I could get them properly verified by sun light. The experiments are therefore still in their infancy, and leave a wide field open for experimenters. My object being to establish a principle, I shall leave it to others to work it out.

89. After having got the apparatus, the first experiment I made was an attempt to obtain blue, and if my anticipations were well founded it was thought that it was merely necessary to cut a small slip of card in the form of a parallelogram, perforate it, place it on the pivot of the small wheel, and then put it in motion over some place near a window reflecting little light from below. But after all was ready I was almost afraid to put it in motion, lest a theory, which had been to me so long the greatest source of pleasure, should, on being subjected to this test, be at last found to be a mere delusion — a phantasy. Even when I had made the experiment and found it completely successful, I could scarcely believe in its reality. I was most anxious to get others to verify what I saw, and one in whom I had the greatest confidence not only for appre-

ciation of colour, but for delicacy of taste in regard to harmony of tints, could at first see little or no colour.

The experiment, however, was successful; blue was obtained by the motion of the slip of white card.

90. The theory I now considered to be established. The only desideratum was to acquire some knowledge of details, some knowledge of the ratio which the shade, or rather penumbra, ought to have to the light for any given colour. This I knew could only be obtained by experiment, and I never doubted but that some approximation would be made to it. I did not, however, anticipate the difficulty of the calculation, nor yet the splendid results at which I have arrived in the production of colour by the motion of a white ray. The experiment is essentially only a repetition of the wafer experiment, the card experiment, and the candle experiment; it consists in *the casting of unrefracted light on a shadow.* Of course the shadow in this case is the darkness over which the white ray moves, and is only cast on the eye as in the wafer experiment, but it is virtually not less a shadow than if it were first cast on a piece of paper. This will presently be made more apparent.

91. After this experiment, being otherwise much engaged, I rested for some time quite satisfied with the result, but on repeating it afterwards I began to think that I had made some great mistake, that there had been an optical illusion, for I could now only obtain purple in place of blue. This last experiment was made in bright sunshine and with a blue sky. In making the same experiment in a dark dull day, during slight rain, again the same purple appeared. I had neglected to note the state of the atmosphere in the first experiment. When, however, in a few days the sky began to clear, and white fleecy clouds to make their appearance, the same blue was obtained as at first. The paper, at least the small slip I used, on a dull day or with a bright

blue sky evidently did not reflect enough of light to produce blue. Blue was obtained best when there were white clouds in the atmosphere.

This is quite contrary to what was to have been expected. Had blue been obtained when the sky was blue there would have been less confidence placed in the experiment; but although it would have created suspicion about the blue, it would not have diminished the value of the fact, that a different shade of colour had been obtained by the motion of a white ray of light. The experiment turning out as it has done leaves a stronger conviction on the mind than ever, that blue is produced by pulsations of light associated with darkness or shadow, and is not, as has been considered, one of the elements of a compound substance, but is rather itself a compound.

At this stage of the investigation, after reflecting much on the subject but without engaging in any more experiments, I was so firmly convinced of the truth of the theory that I very hastily concluded that this experiment alone completely established the vibratory nature of light, and proved, without any question, the homogeneity of the ether. There were no rays in the spectrum so difficult to account for as the blue and the purple, and having discovered the origin and formation of these rays, there would be no difficulty, I thought, in accounting for the others, nor of explaining the nature of the prism, the rationale of the rainbow, and the cause of what are called complementary colours; and after much labour and thought I was led to the conclusion, from this and other experiments, that they may be explained in an easy and simple manner.

Effects of different velocities.

92. Having remarked in the former experiment, that the same slip of paper, moving with equal velocity in different states of the weather, produced different shades of colour,

or rather two different colours, a blue and a purple, I now began to experiment on the effects of different velocities. I found that when I reduced the rate of motion the blue often became purple, but I also found that the same effect was produced when the white ray was carried into the shade, or properly speaking by reducing the intensity of the white ray with which I was operating. In the shade I could not by a more rapid motion raise the purple up to blue, for no rapidity of motion could raise the value of the incident light or the light reflected from the paper; although by reducing the pulsations of the incident light, or the light falling on the paper even in the shade, I could reduce its value.

93. This is the same phenomenon as that of the card painted partly black. In a faint light the black appears through the card as purple; in a stronger light as blue. The one experiment illustrates the other, and both demonstrate the pulsatory nature of light. Of course light must proceed by waves, but there is no proof of length of wave, in the common acceptation of the term according to the wave hypothesis, having any effect in the generation of colour. Nor has there been in this experiment any decomposition of light. The same ray has been used, namely, the white ray reflected from the paper. The frequency of the vibrations has only been increased or diminished. Still it must be understood that the mere increase or diminution of the number of vibrations will not produce colour, or every penumbra would be coloured. But every penumbra is no more coloured than every refracted ray is coloured. Every known substance on which light falls, or through which it is transmitted, casts a shadow, but no shadow can be coloured unless a ray at an angle to the ray which forms the shadow is deflected upon that shadow, as was explained in the candle experiment. Hence the use of refraction. It resolves multiple force; it does not separate quick rays

from slow rays, nor red rays from blue rays, nor constitute fits of easy transmission and easy reflection. But this is not the place to discuss this subject.

94. The experiments hitherto described led to no arithmetical law. I entertained strong hopes, however, that the difficulty would not be great; but when I first began to experiment in the way described I had no idea that the incident light, or pitch of the ether, was so inconstant, so continually changing as I have found it to be. I soon became convinced that nature in this as in other instances never repeats herself, and that every moment of time, could we trace time backwards, could thereby be distinguished from another. I have every reason, however, to think, that by means of motion we shall soon be able to invent some photometer which shall be available for scientific purposes.

95. After having made an immense number of experiments it was found necessary, for the sake of reference, to arrange them under two heads, distinguished by the nature of the motion, viz. 1st, Horizontal; 2ndly, Perpendicular motion.

In horizontal motion the figure, or disc, moves parallel to the machine; in perpendicular motion the figure moves perpendicular to the machine, or parallel to the axis. The phenomena caused by horizontal motion may be divided into three classes:

I. *Class.* When light is reflected from the figure, or disc, representing coloured reflection.

II. *Class.* When light is transmitted through holes in the disc, representing transparent coloured media.

III. *Class.* Coloured shadows, or penumbræ, showing how refraction produces colour. I call these *Solar Spectra by reflection.*

I.—HORIZONTAL MOTION.

1st Class of Experiments.

Coloured Reflection. Theory of light and shade by horizontal motion.

96. If we take a piece of Bristol board (*see plate IV. fig.* 3) in the form of the parallelogram *A B*, and examine it carefully, the light reflected from every spot, as far as the eye can detect, is the same. The space surrounding the card is all darker than the card, and may be considered as shadow. Perforate a hole in the centre *O*, and make it revolve on the centre, then the disc formed by the revolution will not be of a uniform colour. There will be a ring on the disc of a different shade for every radius that can be drawn from the centre of motion on the card. The outer ring will have for its radius half the diagonal *A B*, the next will have for its radius half the side *A D* or *E O*, and the third will have for its radius half the end *D B*, or *O F*. The inner circle formed by the radius *O F*, equal to half the end, will be white, because there is no mixture of shade during revolution, there is no interval in the revolution of the ray; the next, or the circle formed by the radius equal to half the side *A D*, will be coloured, for here there is a mixture of light and shade, and the outer circle will have a darker shade of colour, for there is less light compared to the dark, as may be seen by inspecting the diagram. By making the parellelogram, therefore, revolve we get a variety of colour; but colour is only produced where there is light and darkness repeated at intervals, a result not at all anticipated by science; a result, however, quite in accordance with our course of reasoning.

97. If the parallelogram is made to revolve on the centre *O*, the ratio of the black to the white is apparent to the eye

and may be calculated for each ring or circle on the disc the part within the card being white, the part without black. Hence the ratio of the part within the card, in any of the above circles, to the part without, is that of the white to the black.

98. The number of experiments which can be made with a card of this form is very great and very instructive. It can be made to revolve on its centre, on one of its angles, on the middle of a side, on the middle of an end, or on any point removed from the middle of the side or end; and for every possible change there will be a new figure, having a ring for every distinct radius which can be drawn on the parallelogram from the centre of motion, as well as a modification or change of colour for every change of figure.

Every possible figure, when made to revolve by horizontal motion, takes the form of a disc, and the number of rings on the disc can be calculated on the principle here laid down; that is, there is a ring for every distinct radius which can be drawn from the centre of motion to some well-defined point, and between two such points there is often a gradation of tint similar to that formed by a penumbra.

Examining carefully this experiment, one would at first suppose there could be no great difficulty in determining the numerical value of a ray. The difficulty, however, is greater than it appears at first. The degrees of light are infinite, and the degrees of shade are infinite, and it is, therefore, no easy matter to discover a means of measuring their relative proportions. The smallest change, the most minute modification in a diagram, the least alteration in the pitch of the incident light, or in the angle of the diagram to the incident light, an almost inappreciable variation in the motion of the ray created by the machine, each and all tend to produce a problem extremely difficult of solution.

Circular Figures.

In order if possible to simplify this problem, I tried discs with definite ratios of white and black, and by fixing them at the centre I made them first revolve with

Concentric Motion.

99. These discs were made in this manner: I took a piece of Bristol board, and having described on it several concentric circles, I cut out with a knife, or painted with lamp black, a portion of each of the rings formed by two of these circles, say a half, a third, two thirds, or three fourths, or any other proportions, as in *plate IV. figs.* 4, 5, 6 *and* 7. I then fixed the disc on the machine, and made it revolve in bright light. By this means the white and black portions of the discs were made to pass alternately over the same spot on the retina. The result was, that when revolving neither the white nor the black parts were seen, but the whole disc appeared beautifully coloured with concentric rings, and each ring had its own shade of colour; purples, greens and yellows predominating.

When this method of experimenting is adopted each revolution of the machine may be considered as representing the time or duration of a vibration, and the light reflected from each ring, combined with the velocity, as the momentum of the ray. When the revolutions amount to from six to eight in the second of time, colour is generally formed so as to be seen by the majority of eyes. Some persons can even perceive distinct colours when the revolutions are twice or three times as numerous. At thirty-two revolutions in the second of time, I see no colour, only the geometrical figure generated by the motion and shaded grey, like so many penumbræ, forming the ground, as it were, on which the colours are produced when the light is sufficient.

All that I expected to effect by this method of operating was to produce some tint of green, blue, or purple; and although these colours were produced, and my theory thereby confirmed, still I did not succeed in discovering any definite law, for shades, extremely like each other, are produced by different proportions of black and white on the rings, as well as by different velocities of the machine. Nor is this to be wondered at when we reflect on the infinite variety of greens and other colours which is to be seen in nature. This is indeed an object of research, enough of itself to employ the labour of a whole life.

100. Although I am unwilling to enter on the discussion of diversity of shade at present, I may mention that it requires fewer revolutions per second to produce purple than to produce blue, and fewer to produce blue than green. But it must not be supposed that a colour depends on the number of revolutions of the machine, for every colour is seen on the same disc at the same time.

The coloured diagrams annexed (*plates I., II., III.*) will serve to give some idea of the colours; but the difficulty of producing a representation of such colours as are seen on thin plates, soap bubbles, mother of pearl, &c., will be easily understood.

Excentric Motion.

101. Although by operating with discs having definite ratios of black and white, as in the above examples, shades of various colours were produced, still there was not so great a variety of colour as I desired. Beautiful, therefore, as these colours were, they did not satisfy me. I imagined that if I could by any possibility make one disc revolve over another I might greatly modify the colour. In order to effect this I tried several plans without success. At last it occurred to me that if an excentric motion were given to those figures, or discs, which I

already had, it would virtually be making one disc revolve over another. I therefore cut out a portion — a quarter, or a half — of a ring from the centre of each disc, so that the centre of motion might be made to slide along the diameter at pleasure. By this process the white of one ring would gradually pass on to the black of another, and *vice versâ*. In this way I added greatly to the variety of colour, and, as will be seen on examination, doubled the number of rings which were originally described on each disc. Thus the portion *a a, plate IV. figs.* 4, 5, &c., was cut out, and the centre of motion was fixed at pleasure on any part of the line, or even of the curve.

An examination of the figure whilst in motion will show what takes place. The effect is the same as if there were two discs in motion, one passing over the other; one having for a radius the larger portion of the original diameter, the radius of the other being the smaller portion of the same.

Spiral Figures.

102. The experiment just described having proved very successful, my next device was to employ a sort of spiral figure of white on a black ground. I expected, from the nature of the figure, to obtain very graduated tints. The figures, however, which I at first used were so dark that, although a great variety of colour was produced, the disc appeared like a piece of printed calico, without that gradation which I was in search of. Nevertheless I obtained a very interesting set of colours.

103. Although these colours were not exactly what I was in search of, still the experiment was valuable, for it showed that the plan I was pursuing would lead me to the end I had in view. I therefore drew on the Bristol board two or three concentric circles, cut the central one and a great portion of the others away, in order to give

the figure a spiral sort of form, and at the same time drew a number of rings, so arranged that when put in motion the light might gradually increase in proportion to the shade from the inner part of the ring to its circumference, or *vice versâ*. Some point in the inner circumference of the ring was made the centre of motion as *o, plate V. fig.* 3. Although the figure was of a spiral form before being put in motion, like every other figure moving horizontally, the disc when in motion seemed composed of concentric rings. This experiment gave me nearly all I desiderated in regard to the production of colour. The arithmetical law involved, however, still eluded research. The investigation became difficult, for the smallest change of the centre and the least alteration in the motion created a new obstacle which could not be overcome but by enumerating the elements, and these were unknown, for we have no terms to designate the variety and the shade of a colour.

104. This form afforded a bright yellow, and I hence concluded that a regular spiral would give less yellow and more green. I therefore cut out a spiral of the form shown in *plate V. fig.* 4, having its centre of revolution in *o*. It is so simple that one feels some surprise at the beautiful figure produced by its motion.

One of these discs was made for the purpose of producing yellow, the other to produce green. In a good light they both exhibit very remarkable phenomena. The effect produced by *fig.* 4 *plate V.* when in motion is shown on a reduced scale on *plate III.* The discs themselves were, in general, about six inches in diameter.

Half Discs.

105. It will be observed that half discs are often used in the experiments. I had not experimented long before I found that it was requisite to take much light away, or

rather to produce great intervals between vibrations, in order to obtain good colours. The plan often adopted was to take a whole disc, paint one half of it completely black, and treat the other half in the manner required. By this means I obtained longer intervals between one vibration and another, and saved time in experimenting, that is in making discs.

106. Definite ratios of white and black give distinct colours, because the beats or vibrations are distinct. When the discs are made to revolve with a small degree of excentricity the colours are still distinct, but some of the rings of colour become compounded. When, however, the excentricity is great there is great confusion of colour, proving as I think the comparatively long interval between one vibration and another in nature, in order to produce a distinct impression of colour on the sensorium. (*See plate IV. figs.* 8 *and* 9.)

107. Sunlight is much more inconstant in its effects than gaslight, although the state of the atmosphere has a very decided influence even on gaslight as any one who experiments as I have done will find. In gaslight,

a ring $\frac{1}{4}$ white $\frac{3}{4}$ black produces green or purple.
,, $\frac{1}{3}$,, $\frac{2}{3}$,, ,, another green or purple.
,, $\frac{2}{3}$,, $\frac{1}{3}$,, ,, yellow.

A narrow black line in one direction of the light gives a bright red, in another direction green, whilst in sunlight the lines produce a mauve or an olive green, according to the light or the velocity given to the figure.

But I am unwilling to indicate the numerical value of any colour at present. The subject requires much more study than I have hitherto been able to give to it, so as to yield satisfactory results.

Change of Colour by reversing the motion of the Figure.

108. One of the most singular and remarkable of the phenomena connected with these experiments is the change of colour which takes place by reversing the motion.

Suppose we have a card (*see plate IV. fig.* 8) with concentric rings cut out of or painted on it, and make it revolve by excentric motion, we shall find that one series of rings is coloured reddish-brown, yellow, or some allied colour; another series green, blue, or purple. These rings, when the motion is reversed, change colour. This perplexed me very much at first, for it really indicated something like the polarization of light. A close and repeated examination of the phenomenon indeed convinced me that it must help to remove much of the mystery from what is called polarized light. But this is too wide a field to enter on at present.

109. In order, however, to arrive at the cause of this interesting phenomenon I attempted to analyse one of the semi-discs having concentric rings. I therefore made another semi-disc, divided it into seven concentric rings, leaving a small ring at the centre in order to get the disc fixed as usual. Having drawn the seven concentric rings, I made a new centre, at a distance of half the breadth of one of the rings from the former centre, and drew from it concentric circles so as to meet or touch the other circles, but not to intersect them, as will be seen in *plate IV. fig.* 9. Each ring was thus divided into two equal parts, by a circle passing as it were diagonally across the ring and forming two triangles, one of which was painted black, so that when in rapid motion the ring composed of these two triangles, a black and a white, might be considered as condensed into a parallelogram divided diagonally, or the white and black parts might be considered as the two faces

of a prism, one reflecting light, the other no-light to the spectator. On this disc, when one ring was painted black on the left side of the centre, the next was painted black on the right side, and so on alternately. Hence when in rotation, as the white of any ring diminished the black increased, or *vice versâ* as the white increased the black diminished, and hence the light on the disc appeared as it were stratified, and to come from two different directions, or to be in two different planes.

Now this disc (*plate IV. fig.* 9) when fixed on the machine at the centre, *o*, produced precisely the same effect as *fig.* 8 when fixed out of the centre, as at *e* or *é*, that is at exactly half the breadth of one of the rings from the centre *o*. In both the contiguous rings were of different colours — one being of a yellowish or reddish tint, the next of a purple or greenish tint; and the alternate rings had the same colour. On reversing the motion, in both figures the rings which were yellow became purple, and *vice versâ* the purple became yellow.

110. It will be remarked as a part of this very singular phenomenon, that when the light increases in relation to the black or shadow, it appears *redder* than when it diminishes. In the latter case it appears green or blue, or purple.

In order to analyse the phenomena exhibited by this disc still further, I formed two similar semi-discs, and drew on them seven concentric rings, each of which was divided into two equal parts, as before. But instead of painting each alternate ring on opposite sides of the disc, I painted one disc only on the right of the centre, the other only on the left. The former disc was thus divided into two parts, one representing the right side, the other the left, as in *plate V. figs.* 1 *and* 2. I now expected that the colours which were only repeated alternately on the former disc, would be repeated continuously on these;

that is that one disc would produce only one set of colours, the other disc the other; and when the motion was changed that each would represent the colours of the other. I was delighted with the appearance as well as the success of this experiment when seen by gaslight. The same effect is produced in good sunlight.

111. It may be mentioned here that all these experiments were originally made by gaslight. I may also add that it would be useless to describe the colours of any of these figures, as they vary so much, not only from the nature of the atmospheric light, but according to the spot which is made the centre of the motion. For instance, when *fig.* 9, *plate IV.*, already described, was made to revolve excentrically in daylight, the sun shining white through a haze, and the ground being completely covered with snow, there appeared two series of the most beautiful greens and purples imaginable.

112. It is to be remarked that though the amount of light and shade on these two semi-discs *(plate V. figs.* 1 *and* 2) is the same, still, when they are put in motion, the colours are different. The ratio of light to shade would thus appear to be no more the cause of difference of colour than the refrangibilities of the rays, or the lengths of waves. Other circumstances must be taken into consideration, such as whether the vibration is one belonging to an ascending or a descending series, to an increasing or a decreasing force. But what is this, if it is not the ratio of light and shade? I shall return to this subject again under the head of perpendicular motion.

Now if we consider the diagonal of a ring, as dividing it into two triangles, one being white, the other black; and if we suppose the white triangle to represent a beam of light and each revolution of the machine to correspond to a vibration of light, the angular point being the commencement of the vibration, and the third side or base

its termination, or *vice versâ*; if we make this supposition, we shall have no difficulty, on examining the motion, to account for the change of colour on the change of motion, however strange the phenomenon may at first sight appear. The *direction* of the ray of light is an important element in all these phenomena. When the light increases, or, which is the same thing, when it comes after the shade during the motion of the disc, then the colour is red or allied to red. When the shade follows the light, or which is the same thing, when the light decreases, the colours are allied to blue; and all colour is only a modification of these two conditions of the ether or a combination of the two. No more has hitherto been made of the subject of colour, analyse it as much as we please. Every sensation is reduced to one or other of these two conditions, infinitely diversified by an infinite wisdom.

113. The experiment just described I consider as not only very remarkable and very beautiful, but also as highly instructive. There will, however, arise in the minds of some who have long and profoundly studied the subject of light, and who are much more conversant with its details than I am, but who are wedded to the science of the day, the question—Is not this, after all, only the effect of the polarization of light? I am not about to discuss this point at present, but I would ask those who are unwilling to accept new ideas to explain these phenomena on any old hypothesis; for until this is done, they must for ever remain a stumbling block in the path of science. They are incontestible facts, not illusions, and cannot be explained away by any empirical law or antiquated scientific conventionalism.

Remarks.

114. Although by operating with discs, having definite proportions of white and black on them, colours were pro-

duced, they were not so defined as to afford any basis for a unit of measure. The experiments only proved the necessity for assuming a negative element in colour.

The experiments by excentric motion introduced a wider range of phenomena, and showed how this wider range was obtained: but the colorific power of a molecule of light is to me still as immeasurable as its weight is imponderable.

The spiral figures showed perhaps still more distinctly than the excentric how one ray modifies another, and how gradation of tint is produced.

115. When treating of the colours produced by reversing the motion, I have shown how an increasing series of rays produces one species of colour and a decreasing series another.

If then we are still left without a mathematical element for expressing the value of a ray of light, we are not in a worse condition mathematically — at least as far as colour is concerned — than we have ever been. For never since the science of light has been systematically taught has the value of a ray been expressed mathematically. All such calculations are mere empiricism, and of no practical utility.

116. If we take a piece of Bristol board, and draw on it a spiral figure in china-ink or lamp-black, or draw on it several concentric black circles, and make the disc revolve with excentric motion, the black circles or spiral in gas-light will appear a brilliant red. On reversing the motion they will appear green or blue, the effect depending much on the light.* (*See plate IV. fig.* 9.)

* To prove that there are, as it were, two discs in motion, and each different in colour from the other when the motion is excentric, let us make a semi-disc of about six inches diameter, and draw on it several concentric rings. Make a few of the lines darker than others to give variety. Let *o* be the centre, and *x* the centre of motion. The two discs will have for radius *x a* and *x b*, and the two series of rings will be dis-

117. Shall we then call the unit of measure for light red? If this, as seems probable, is the case, what then is green or blue? When I come to consider the phenomena of the prism I shall recur to this subject. To speculate, however, on the theory of light, as has been hitherto done — making one supposition to help another, and one emendation to mend another — is a waste of time and of talent. Could such a measure as we are in search of be obtained, it would be of incalculable value to science. Hypothesis is dangerous here. Besides, if these experiments are as conclusive as I consider them to be, they prove that the mathematical method of reasoning, so often recommended by Newton in his *Optics*, has as yet done nothing for the science of colour. But I shall be told, that it is better to have a mathematical equivalent for the value of a ray, even although erroneous, than a mere physical process without one. Well, be it so to those who think so.

HORIZONTAL MOTION.

2nd Class of Experiments.

Transmitted Light, representing Coloured Media.

118. The light in these experiments was transmitted through open spaces in the figure or disc.

For the sake of distinction I call the experiments which I am about to describe experiments on transmitted light. In the first class of experiments, when the white of the

tinctly marked by their colour — one series being blue or green; the other of a brilliant red, tinged or edged with orange and yellow. In sunlight the colours are more subdued. On reversing the motion, the two series instantly change colour. This may be considered as another or further analysis of *fig.* 9, or 8, on a smaller scale. By making rings of various breadths, such as are here shown, to combine, we get a great variety of colour. We may be able, perhaps, to compound any colour in process of time by careful experiment and analysis.

paper was made to represent the white ray of light, the black might be painted on the disc, or the spaces cut out as fancy dictated, for the open spaces appeared black from reflecting no light. In this class of experiments, only discs with open spaces could be used, as the white of the sky was here made to represent the white ray, and was therefore transmitted through open spaces. The whole disc, after the parts were cut out which were to allow the passage of light, was sometimes painted black, but the shadow cast by the paper itself, when held between the light and the eye, I found to be alone sufficient to produce colour.

Half discs could seldom be used in these experiments. In my former experiments it was found that to obtain distinct colours it was necessary to destroy a great portion of the incident light; consequently half discs were preferable to whole ones. In this class of experiments, in order to destroy a necessary portion of the incident light, whole discs were employed in preference to half discs. For not only the light and shade on the discs but the method of using them was different. In the former method the parts cut out were dark; in this they were white. When employing the former method the light was reflected from the disc; in this the disc was held so as to be made to revolve between the eye and the light, and the light was transmitted.

I at first experimented with these discs by making them revolve between my eye and a white cloud, but as there were seldom white clouds to be seen, I was forced to look for something to imitate a white cloud, so that I might be able to experiment at any convenient time. Simple as such an invention may appear to an inventive mind, it was some time before it occurred to me that a white sheet of Bristol board might at times be made to supply the want of a white cloud if there were otherwise a sufficient quantity of atmospheric light. In sunshine or

in gaslight, then, a sheet of white paper could be used in place of a white cloud, and the different effects produced by changing the angle of the incident light, that is by reflecting on the disc either more light or less light as might be thought proper, became most instructive, much more so than with the light from the white cloud, which is less manageable.

119. The method of proceeding was as follows: having placed the disc on the wheel, and made it revolve before a white cloud, or above a sheet of white paper highly illuminated, the holes cut in the disc permitted the light from the cloud or the paper to pass through them as the disc revolved. The light from the cloud or paper being intercepted at intervals by the opaque parts of the disc, colour was produced on the principles already explained. The change of colour, which results from a change in the quantity of light, caused by a change of the inclination of the paper to the light incident upon it as well as the disc, explains many natural phenomena.

I may mention that a sheet of ground glass hung up before a window may be used instead of the cloud or paper: semi-discs may be employed before such a sheet of glass.

In many cases also the light reflected from a mirror answered better than the light of the sky. When the ground is covered with snow these experiments succeed very well.

HORIZONTAL MOTION.

3rd Class of Experiments.

Solar Spectra by Reflection, or Coloured Shadows.

120. In the experiments which I am now about to describe I made use of the apparatus employed in the second

class of experiments. It is the shadows which these discs cast on the white wall of a room, or on a white sheet of paper, which were investigated.

On taking one of the discs of the second class of experiments, that is discs with open spaces to transmit light, and placing it so that it should cast its shadow, either in sunshine or in gaslight, on a sheet of white paper, and then making it revolve, the shadows were seen vividly illuminated. The colours in this class of experiments were more distinct than those of the other two classes. They exhibited, however, precisely the same phenomena under a different phase, but they had this advantage, that they could not easily escape the notice of the most careless observer, for they had the appearance of being painted on paper, in the same way as the solar spectrum appears painted; hence I call them solar spectra by reflection.

121. If the experiments are made under a jet of gas, the shadows will have very marked penumbræ before being put in motion, and if when in motion the penumbræ, which are now coloured, are carefully examined, it will be seen how closely such phenomena are allied to the colours produced by prismatic refraction. In fact they demonstrate that the colours of the prism are coloured penumbræ. They give a complete proof of the identity of the two phenomena.

122. Newton attempts to prove that the penumbræ have nothing to do with the colours of the solar spectrum. He is constantly reverting to the proposition which he is straining his faculties to demonstrate, "That colours are to be derived from some other cause than the new modifications of light by refractions and shadows." He acknowledges that there is a "penumbra," to use his own words, "made in the circuit of the spectrum Y," that is before refraction, "and that penumbra remains in the rectilinear sides of the spectrums P T and p t," that is after refrac-

tion. He says: "I placed, therefore, at that hole a lens or object glass of a telescope which might cast the image of the sun directly on Y without any penumbra at all, and found that the penumbra of the rectilinear sides of the oblong spectrums P T and p t was also thereby taken away, so that these sides appeared as distinctly defined as did the circumference of the first image Y." But what does this prove? It only proves that there was not so much penumbra on the sides of the spectrum when the lens was used, but it does not prove the same as to the ends, the direction in which the light is refracted by the prism. There is little dispersion or refraction on the sides even at first; the dispersion is transverse, or towards the thick part of the prism, not longitudinal, or towards the ends of the prism. But how could Newton himself be satisfied with this experiment when a little farther on in his work he elaborately attempts to prove the impossibility of forming an achromatic glass? He says: "Seeing, therefore, the improvement of telescopes of given lengths by refractions is desperate, I contrived heretofore a perspective by reflection, using instead of an object glass a concave metal." This admission of itself proves that Newton was not experimentally in a condition to say that the object glass removed all penumbra. It is known that light, in passing the edge of a solid, always causes a penumbra greater or less according to the distance of the luminous body from the object, or of the object from the place on which the image is received. This penumbra may be either a graduated grey, or coloured according to circumstances. It is these circumstances which we are now investigating.

123. I shall quote another experiment of Newton's in reference to this point (*Optics*, p. 102):

"*Experiment* 3. Such another experiment may be easily tried as follows: Let a broad beam of the sun's light

coming into a dark chamber through a hole in the window-shutter be refracted by a large prism *A B C*, whose refracting angle *C* is more than sixty degrees, and so soon as it comes out of the prism let it fall upon the white paper *D E* glued upon a stiff plane; and this light, when the paper is perpendicular to it as it is represented in *D E*, will appear perfectly white upon the paper: but when the paper is very much inclined to it in such a manner as to keep always parallel to the axis of the prism, the whiteness of the whole light upon the paper will, according to the inclination of the paper this way or that way, change either into yellow and red as in the posture *d e*, or into blue and violet as in the posture δ ϵ. And if the light before it fall upon the paper be twice refracted the same way by two parallel prisms, these colours will become the more conspicuous. Here all the middle parts of the broad beam of white light which fell upon the paper did, without any confine of shadow to modify it, become coloured all over with one uniform colour, the colour being always the same in the middle of the paper as at the edges, and this colour changed according to the various obliquity of the reflecting paper, without any change in the refractions or shadows, or in the light which fell upon the paper. And therefore these colours are to be derived from some other cause than the new modifications of light by refractions and shadows.

"If it be asked what then is their cause? I answer that the paper in the posture *d e*, being more oblique to the more refrangible rays than to the less refrangible ones, is more strongly illuminated by the latter than by the former, and therefore the less refrangible rays are predominant in the reflected light. And wherever they are predominant in any light they tinge it with red or yellow, as may in some measure appear by the first proposition of the first book, and will more fully appear hereafter. And

the contrary happens in the posture of the paper $\delta\ \epsilon$, the more refrangible rays being then predominant which always tinge light with blues and violets."

Newton appears to have been too easily satisfied with his experiments on penumbræ, for this experiment seems to me to prove the very reverse of what he supposes. I consider that the doctrine of more and less refrangible rays completely fails him here, and I find from a note which I pencilled on this part of his *Optics* many years ago that this was my first impression. I, however, never ascribed this to anything but my inability to comprehend the profundity of Newton's doctrine of light. But nature herself speaks to me now in a different language to that of Newton. When we incline the paper to the rays, as directed by Newton in this experiment, we do two things; we remove a portion of the paper farther from the prism, and thus increase the penumbra; and at the same time we cut off nearly one half of the rays which fell directly on the paper, when it was kept perpendicular, and thereby diminish the light. My experiments lead to the conclusion that it is necessary not only to have penumbræ in order to obtain colour, but that it is also necessary to diminish the momentum of the incident light, similar to what is done in this experiment of Newton's. But further, if we consider the paper as an eye looking into the prism, a little study will soon convince us that at every change of the angle the object seen from the the paper is totally different, that the lights and shadows are completely altered. We have merely to trace the lines of the image backwards through the prism to convince ourselves of this.

124. Though additional proof may be required by others, I am satisfied that my experiment proves that the spectrum is caused by coloured penumbræ in the way I have shown, and consequently solves the difficulty of its length compared to its breadth. It even solves the difficulty which Newton

is here contending with, viz.: the cause of the spectrum being white when it first emerges from the prism.

125. Let us now revert for a moment to the phenomena of Class II., caused by what I have called transmitted light, and they will be seen to be analogous to those produced by transparent coloured media. The holes in the card may be considered as representing the pores of the substance, and the change of the angle of the paper reflecting the light, or of the figure transmitting the light, may be considered as corresponding to the difference of arrangement in the molecules of different solids, which is the cause of their refracting (as it is usually termed) light differently.

The experiments of Class III. are similar to those of Class II.; in one case the images are depicted on paper, in the other directly on the eye. But in those of Class III. the penumbræ are seen on the screen before motion is given to the machine. In Class II., although the penumbræ are there, before motion is produced, they are not perceived by the eye. I consider the phenomena of Class III. to be similar in many respects to prismatic phenomena, and when we compare those of the second and third classes together, we can understand why penumbræ are not seen in the prism, and at the same time learn how essential they are to the formation of colour.

These two classes of experiments clear away much of the mysterious nature of the prism. When we come to the phenomena caused by perpendicular motion, the subject will be made clearer still. Other difficulties will be solved, but the prism would require a paper for itself.

I have not thought it necessary to give any diagrams, coloured or otherwise, to illustrate Classes II. and III. of experiments. They are similar to those already given, as far as colour is concerned.

PERPENDICULAR MOTION.

Experiments in Imitation of Transparent or Crystalline Solids.

126. The experiments hitherto described were all made by employing horizontal motion. By changing the motion from horizontal to perpendicular, I have endeavoured to imitate solids, and show other effects seen in the passage of light through transparent media.

It will scarcely be necessary to premise that the revolution of a parallelogram by perpendicular motion produces a cylinder, that the revolution of a triangle produces a cone or a double cone, and that the revolution of a semicircle produces a sphere. In fact this class of experiments affords an immense variety of figures which an ingenious and inventive mind may diversify to any extent.

127. Those who understand the principles already explained will easily understand what is to follow. The principles are entirely the same, but in some of the experiments colour, from the nature of the generated figure, is seen more distinctly than even in the former experiments to be caused by very sensible and easily counted beats. Although the principles involved are extremely simple, it may be advisable before proceeding further to advert for a moment to

The Theory of Colour by Perpendicular Motion.

128. Let us take in our hand a white card *A B*, (*see plate V. fig* 5,) made of Bristol board, and let us examine what we see when we hold it by one end. If it is kept between the eye and the floor the whole of the card appears white, and the space around it dark. Divide the end into two equal parts at *a*, place the middle point exactly on the pivot of the wheel, and make it rotate. No

colour will be seen, for the one half fills up the space occupied by the other during rotation; there is no mixture of light and shade. The white does not encroach on the dark space surrounding the card.

129. Now change the centre of revolution of the card from the middle of the end to some point nearer one of the sides, say to *b*, at a distance of one fourth of the end from the side *A C*. A portion of the larger division equal to the difference between it and the lesser, in this case equal to one half of the end of the card, will in every revolution encroach upon the surrounding dark space, and will consequently produce colour; or in every revolution the opposite side of the portion *b f* will fill up the space *C e*, which is equal to it, and the opposite side of *C e* will fill up its equal space *b f*, and hence there will be, so to speak, two beats of light on *C e* or *b f* for every beat on *a D*, or its equal *C H* when in revolution.

130. But if we paint *C e* black, only the middle portion *b f* will have double beats, and if we paint the reverse or other side of *b f* black, making *b* still the centre of revolution, the whole card will have the same number of beats. On this principle a great many diagrams were formed.

131. Continuing to operate with the parallelogram, it may now be made $\frac{1}{8}$ or $\frac{1}{16}$ of an inch in breadth. If we place the edge *A C* or *D B* quite perpendicularly on the centre, and in a line with the axis of rotation, we shall obtain a line of greatly concentrated light. I first attempted to obtain red in this way, and often did, but have been more successful in getting a delicate pink, approaching to a rich reddish-purple.*

* When Bristol board is used it is difficult to make it remain perpendicular for any time, as the centrifugal force almost instantaneously gives it an angular motion. Some other material should be used. I have tried silver, but my instrument is not well adapted for holding it firmly, and I have not experimented much with it.

For perpendicular experiments, the part *a* of *fig.* 2, *plate IV.*, is taken off and *b* is put in its place. *b* is composed of two plates, with a screw to hold fast the cards used.

132. If the edge be removed from the centre, there will be less concentration of light when in motion. The edge will have a larger circle of revolution, and we shall now have greens and other allied colours, such as olives of various shades. This indicated something like a system of waves, but attentive consideration of the phenomenon would rather lead to the belief that the angle of reflection had some effect, and no doubt it has, in modifying the light, but not in decomposing it, or resolving it into its simple elements. There is not only a different quantity of light reflected from the edge in motion at every angle, but the circle of revolution is greatly widened, and the ratio of the dark to the light immensely increased. As far as I can perceive, therefore, the very same law is in operation here as in the case of the plane discs — the law of vibrations or pulsations, and vibrations or pulsations which can be counted. It must be inferred, however, that if the motion of light is so exceedingly rapid as it is found to be, the length of wave must also be exceedingly large, otherwise the vibrations would be so frequent as completely to destroy colour.

133. In conducting experiments by means of perpendicular motion, it is not necessary that the figure should have the form of a parallelogram; I only selected the parallelogram as the easiest figure for illustrating the theory and the plan of operation. Any figure may be used, or figures may be cut out of the card, or painted on it, as in the former experiments. Different figures may also be painted on different sides of the card, and a mixture of tints as well as very pleasing effects produced in the manner already described.

134. The number of various figures which may be made in this way is endless. I will content myself with making a selection of such as will enable us to explain some of the more familiar optical phenomena.

Experiments.

Take a card and describe on it a semi-circle; it may be either cut out of, or painted on it. (*See plate V. fig.* 6.) Fix it on the machine, so that the diameter *a b* may be perfectly in a line with the axis of motion. When in motion the semi-circle will appear a circle. There will be a line, *a b*, of bright colour along the diameter, equal in breadth to the thickness of the card, if the semi-circle is cut out. Should the diameter *a b* be removed in the smallest degree from the centre of motion, the diameter of the generated figure will show various shades of purple and green. On each side of the diameter, the semi-circles of which the generated circle appears to be composed, will be coloured, not unlike a peach, beginning at the diameter with purple, then approaching to yellow, and verging to green towards the circumference.

This is another demonstration of the correctness of my views with regard to the change of colour which takes place on reversing the motion of some of the plane figures, for in this experiment the light on each semi-circle visibly proceeds from two different quarters. One, however, is more apt to be struck with the phenomenon when horizontal motion is employed.

135. To vary this experiment, in place of a half, cut out a whole circle. In this case there will be a black line along the diameter where there was formerly a bright line, but it will be shaded off on each side towards the circumference as before. When the figure thus generated is minutely examined it will be seen to consist of a hollow globe, and the shadows from the edges of the card will distinctly appear on the back of the generated figure. The shadow on the aerial figure formed by the card is very interesting. If a circle painted black be used this appearance will be wanting.

136. Another variety of this experiment may be made by cutting out two semi-circles, making the circumference of the one touch the other, and fixing the figure so that one of the diameters may be in a line with the axis. (*See plate V. fig. 7, and coloured diagram, plate II.*) The figure is fixed on the axis of the machine at *a*.

The appearance is precisely the same as before, as regards colour. The form differs in having two semi-circles besides the circle. The semi-circle, which has its diameter fixed on the axis of motion, will produce a circle, the other a semi-circle on each side of the circle. The whole figure when in motion has the appearance of two cylinders, one within the other. The reason is obvious.

In this figure there are two small circles cut out at a distance (from the centre of each of the larger circles) equal to the radii of the larger circles. These were made to assist the eye in following the motion of the larger circles, and as the figure generated is rather remarkable they have been retained. Observe the difference in colour of these small circles in the outer cylinder and of those in the inner cylinder. In the outer the colour is bluish, in the inner red, showing a bright line up the centre caused by the small bit of Bristol board connecting the two semi-circles, as may be seen by examining the generating figure. (*See plate V. fig. 7.*) In the inner cylinder the motion of the small circle is traced receding as far as the eye can follow it. It will be observed that the colour is when receding nearly the same as in the outer cylinder, but partaking more of green.

137. Take another card, and cut out parallelograms, all of the same or of different breadths. (*See plate V. fig. 8.*) The parallelograms on the right and left of the axis will be coloured alike, as in the semi-circles. I have often imagined the colour of these to resemble clouds, and the different parallelograms to represent the stratification of

the atmosphere. Others may be prevented from forming the same idea in consequence of the too exact geometrical forms which the different strata assume in the experiment.

Supposing the beam of light from the card to generate a prism, or the section of a lens, one on each side of the axis, we may then imagine the parallelograms on the left side of the axis to be figures seen through the left side of the axis of the prism, and those on the right to be figures seen through the right side of the axis; that is through the two angles on the right and left of the axis of the prism. They obey the same law as regards colour.

Is not this a most important step towards an explanation of prismatic refraction?

138. These experiments may be infinitely varied. There is, however, another which I cannot avoid mentioning.

On a piece of pasteboard draw two circles. (*See plate V. fig.* 9.) The outer circle may be three and a half inches in diameter, the inner a quarter of an inch or so less, which will give a ring about an eighth of an inch broad. Draw two lines, one on each side of the diameter, so as to inclose a space of about $\frac{1}{16}$ of an inch broad. Cut the circle, *a a a*, out of the board, and the segments *b c* out of the circle, leaving the space *d d* as a diameter. Fix the figure at *o* on the diameter, either perpendicularly or at an angle, and examine the different effects.

When made to revolve in sunshine the colours at times are most exquisite. The whole figure is as elegant as imagination can conceive. It is not so opaque as mother of pearl, or as soap bubbles, but it partakes of all their evanescent colours. Attention is particularly directed in this experiment to the shadows which are seen on the generated figure, cast by the edges of the figure itself. They are very remarkable, and illustrate, I suspect, many of the phenomena which are to be seen in crystals and

sections of crystals, when light is allowed to fall upon them in one direction only.

139. There is another variety of this experiment. Make a similar figure, but in place of cutting out any of the segments, cut the arc of one completely through, but the chord or diameter only half through the thickness of the board, so as to enable it to be bent backwards. Bend it so as to be at right angles to the other segment. The effect of this is very beautiful, and the generated figure extremely elegant. It may be mentioned, by the by, that all the geneated figures are beautiful, and may be called gems of art.

140. From a great variety of coloured diagrams three have been selected to be lithographed as specimens of the figures as well as of the colours produced by these experiments. *Plate I.* is a representation of the effect produced by *fig.* 10*a and* 10*b, plate V.* In a paper such as this where the subject is only briefly, and for the first time presented to the public, I could have wished that some more simple figure, more like a cut crystal, had been selected. But as it is, let us consider *A* and *B* as the obverse and reverse sides of a card, and that they represent, when in motion, a transparent solid body, being cut so that when held with the obverse to the sight the dark part appears as a plane or planes representing no-light, and the white part as a plane reflecting light; and, *vice versâ,* when the reverse is held to the sight the part which was white before appears now to be black or dark, and what was dark before to be white. Centre the card exactly on the point *o,* so that when in motion it may be perfectly perpendicular, the obverse repeating itself precisely on the reverse and the reverse on the obverse, and we shall obtain a figure coloured as in *plate I.*

The principle on which the colours are produced is as easily understood from this as from a figure of more crystalline form. *The different colours are obviously produced*

by gradations of light and shade, the pulsations of light being repeated at greater intervals than science teaches, and do not depend on rays having different degrees of refrangibility, nor on rays or waves, some of which are repeated in a given time more frequently than others.

141. These experiments, taken in *cumulo,* show us light in two different states, in an increasing and in a decreasing state. To the former belong red and its allies, orange and yellow; to the latter blue and its allies.

The sensations produced by these two states of the ether may appear astonishing to some because they are novel, and make what were formerly only ideas or conceptions of the mind appear, as it were, palpable to the senses. For everything which is seen is considered to be palpable or material. But on due reflection they are not more wonderful than other operations of nature. Who would anticipate that heat and cold are but modifications of one law? For we say that the sensation of heat arises from an increasing or absorbing operation; that of cold from a decreasing or radiating operation. We even speak of colours as warm and cold. We call red and its allies warm, blue and its allies cold. Green is warm when it partakes largely of yellow, cold when blue predominates; the colours arranging themselves, according to our sensations, under the law which we have discovered.

But we must lay speculation aside for the present. The metaphysics of every operation ends ultimately in the unconditioned; the final cause merges in the infinite.

142. At this stage of our inquiry there naturally arises an extremely interesting question, viz: How does nature produce these penumbræ, this gradation of light and shade of which we have been speaking? To enter into all the minutiæ, and to attempt an explanation of the whole circle of optical phenomena, would be impossible; every one must apply the principles of the science for himself,

and I have no doubt that in the hands of skilful investigators many things will be made plain that are at present hidden and mysterious. I may, however, repeat what I have oftener than once remarked, that a wave of light when it falls on a substance composed of transparent plates placed at different angles to the incident light, has its force decomposed by the different plates or laminæ. The wave or pencil, in falling on the first surface, is partly reflected and partly transmitted, and the ray transmitted by the first plate may be also partly transmitted and partly reflected by the next, and so on any number of times; but the second plate not being at an angle to reflect all the light incident on the first, performs the part of shadow to the vibrations of light on the first surface; and again, at another angle, the first surface may act as a shadow to some other of the plates which form the reflecting substance, or *vice versâ*; causing a change of colour at every change of angle, and giving to the substance a changeable or flitting appearance of colour. In this way there is an analysis of the pencil of incident rays, and thus colour is produced in a manner similar to that of our experiments. Just as our experiments deprive the incident light of many of its rays, so do these laminæ or plates in a similar manner perform the same operation. They give us our two elements of colour, light and no-light; the two coördinate principles, the active and the passive.

143. When we combine the conclusions deducible from these experiments with what we know of the geometrical construction of some of the natural objects surrounding us, we learn with what beautiful, with what sublime simplicity nature performs operations which science complicates and involves; how phenomena which in the hands of a human architect — in the hands of a Newton — are made to appear intricate and abstruse, are in reality produced with an ease and an elegance — if such artistic expressions can be

allowed — which no created being, no one but a divine architect could contrive or conceive. For example:

The phenomena of thin plates which in the hands of Newton have received a philosophical importance, from having led him to invent his celebrated theory of fits of easy transmission, and fits of easy reflection, is by the experiments on the chromascope brought more within the range of investigation. In producing these phenomena nature simply places two or more transparent plates in different planes, or at different angles, to the incident light, the one plane reflecting no-light, or virtually causing a shadow, the other plane reflecting light, and colouring that shadow, by this simple expedient performing the same operation which we attempt to do by our diagrams in motion. Our motion produces on the light not only the effect of vibrations, but the light and shade, when in motion, produce an effect similar to that resulting from the geometrical construction of a natural body, composed of thin plates. In our method of operating, a slow motion gives rise to one colour, and a quick motion to another; but the natural process yields the same results by different means. Nature analyses force; it does not resolve a compound substance into its constituent parts; on the contrary, it may be rather said to compound a simple substance; or in other words, natural objects are all constructed so as to reflect or transmit more or less of this impinging force, which we call light, and all the colours in nature are produced by the combination of one simple uncompounded substance with the negative element, which is now demonstrated to be not only an essential but a coördinate element of colour with the positive.

144. The celebrated phenomena of soap bubbles, which have amused the young and the old by the charming play of colours on their surface, receive also an easy solution by the same process of reasoning.

Newton explained the play of colours on soap bubbles by their ever varying thickness. He protected them, and watched them with care, and the result of his reasoning, and the uses he put it to, are generally known. I do not mean to follow him in his observations, or in his arguments, for our principles of reasoning have nothing in common. I wish, however, to say that twenty years ago, I examined most carefully the same phenomena, and although I have not repeated my observations since then, they made such an indelible impression on my mind that nothing can remove it. When examining the play of colours on the bubbles I remarked, what every one who has attended to the subject must have done, that there was a motion caused by the action of gravity, and also a motion attributable to that of the air within the bubble. The air, within and without, being of different temperatures, there must necessarily be motion, either until the bubble bursts or until an equilibrium is established; or should there be sufficient tenacity in the infusion, these waves or circles remain stationary for a time. These two motions, or even one of them at a time, are quite sufficient on our principles of reasoning to explain the difficulties attending this hitherto intricate subject; for it becomes only another case of the same kind as that of thin plates. The motion within makes the little waves or facets move in a different direction to those on the outer surface of the bubble. We have, therefore, two transparent planes or surfaces reflecting light, the one from the eye, the other to the eye; the one causing a shadow, and reducing the number of vibrations of the incident light, the other illuminating that shadow.

From these two notable phenomena of thin plates and soap bubbles is to be derived the law which regulates the production of colour in many other natural objects.

145. The doctrine of complementary colours, as taught

by science, does not explain the phenomena of the two wafers. The phenomena of the two wafers, however, as now developed by experiment, afford a simple and easy solution of the nature of complementary colours. When any spot on the retina is affected to an unusual degree by looking for some time at a vivid colour, that spot of the retina gets excited beyond any other portion of the retina. When, therefore, the eye is shut, the extra excitement continues, and as the excitement is a vibratory excitement, in the intervals shadow or rest (if I may use the expression) is perceived or felt, and the image becomes differently coloured to what it was; and when the eye is again opened the excitement remains, but the colour changes with the colour of the spot on which the image is seen. There is a diminution of force in the one case and an augmentation of it in the other.

My experiments, I humbly conceive, elucidate the phenomena we have been considering. In the whole discussion of the subject I have not been compelled to resort to any supposition, to require any exercise of faith. I have not even asked the belief in a single axiom. The experiments have supplied axioms and arguments, and the most complete analytical proof which can be given — the production of phenomena which can only be explained by my argument; and I think that they completely set aside the theory which is based on the doctrine of the different degrees of refrangibility of the rays of light, and must modify to a great extent the wave theory as at present taught.*

* Where I use the expression "number of vibrations," "momentum" might be thought more appropriate, but neither conveys the exact idea wished to be communicated. For "increasing or decreasing force" I might have used "converging or diverging rays," but these terms are used in a sense not identical with the received one. The "motion of a white ray" may be considered as synonymous with the "periodical interruption of a white ray."

DESCRIPTION OF THE PLATES.

PLATE I., coloured, is *fig.* 10*a*, and 10*b*, *plate V.*, as seen when in motion.

PLATE II., coloured, is *fig.* 11, *plate V.*, as seen when in motion.

PLATE III., coloured, is *fig.* 4, *plate V.*, as seen when in motion.

PLATE IV.—*Fig.* 1, *vide* page 58.

Fig. 2. Form of machine. Small wheel makes eight revolutions for one of large; *a*, nut for fixing on the discs; *b*, nut for fixing figures when the motion is perpendicular. They are moveable, being merely screwed to the spindle.

Fig 3. Diagram to explain theory of horizontal motion.

Figs. 4, 5, 6, 7, 8, 9, and *figs.* 1 and 2 in *plate V.*, various forms of discs and half discs described.

Fig. 8, a disc with several concentric rings, white and black alternately.

Fig. 9, and *figs.* 1 and 2 in *plate V.*, are an analysis of the preceding *fig.* when made to move by excentric motion.

Fig. 9 is a semicircle similar to *fig.* 8. Several concentric circles are drawn. Then with another centre, one half the breadth of one of the rings from the former centre, other circles are drawn so as to divide each ring into two, diagonally. Half of each ring is painted black. If the half of the first is painted black on the right, the half of the next is painted black on the left, and so on alternately. When revolving on first centre it produces an effect similar to *fig.* 8 when excentric. Each alternate ring has a different colour.

PLATE V.—*Figs.* 1 and 2 are a further analysis of *figs.* 8 and 9, *plate VI.*

Fig. 1 produces one series of colours; *fig.* 2, the other series; or by reversing the motion, the one disc produces the same effect as the other. *Fig.* 1 when moving from left to right, as the hands of a watch, produces a reddish-brown, shading off to yellow. *Fig.* 2, when moving in the same direction, produces a kind of gloomy green. In the one the light begins bright and becomes fainter; in the other it begins faint and becomes brighter.

Fig. 3. A sort of spiral to produce gradation of colour. From the predominance of the white, when the light is increasing, the ring on the rims is a bright yellow; at times very bright.

Fig. 4. A spiral, for a similar purpose as *fig.* 3, only the shade predominates, and the rim is consequently of a beautiful green. These produce splendid effects, both by perpendicular and horizontal motion. (*See plate III.*)

Fig. 5. Diagram to explain the theory of perpendicular motion.

Figs. 6, 7, 8, 9, are merely varieties of *fig.* 5; the explanation of which must be kept in view when studying the effects produced by these figures.

Figs. 10*a* and 10*b*, when revolving produce the appearance represented on *plate I.*; *A* and *B* are the obverse and reverse of the card.

Fig. 11, when put into motion produces the coloured image shown on *plate II.* The obverse and reverse of this card are identical, as the black parts are cut out.

The horizontal discs are reduced in size; those by which the experiments were made were about six inches in diameter.

II.—*On a Method of Testing the Strength of Steam Boilers.* *By* J. P. Joule, LL.D., F.R.S., *&c.*

Read November 29th, 1859.

In the course of my experiments on steam, I had to employ pressures which I did not consider absolutely safe unless the boiler was previously tested. The means I adopted, being simple, inexpensive and efficacious, may, I think, be recommended for general adoption. My plan is as follows:—The boiler is to be first entirely filled with water, care being taken to close all passages leading therefrom. A brisk fire must then be made under it, and, after the water has become moderately heated, say to 90° Fahr., the safety valve must be loaded to the pressure up to which the boiler is intended to be tried. Bourdon's circular gauge, or other pressure indicator, is then to be constantly observed; and if the pressure arising from the expansion of the water goes on increasing continuously, without sudden decrease or stoppage, until the testing pressure is attained, it may be inferred that the boiler has sustained it without having suffered strain.

In testing my own boiler, the pressure ran up from zero to sixty-two pounds on the inch in five minutes. It rose more rapidly at the commencement than towards the termination of the trial, owing to leakage, which was considerable, and of course increased with the pressure. But as there was no sudden alteration or discontinuity in the

rise of pressure, it was evident that no permanent alteration of figure or incipient rupture had taken place.

In the so-called testing by steam pressure it is impossible to be sure that a boiler has not thereby suffered strain; and there is therefore no guarantee that it will not burst if subsequently worked at the same or even a somewhat lower pressure. It is to be hoped that this practice, objectionable on account of its uselessness as well as its danger, will be immediately abandoned.

In the ordinary hydraulic test the water is introduced discontinuously, and therefore the pressure increases by successive additions, rendering it difficult to be sure that strain is not taking place. This system also requires the use of a special apparatus.

The plan I recommend is free from the objections which belong to the others, and the facility with which it may be employed will probably induce owners to subject their boilers to those periodical tests the necessity for which fatal experience has so abundantly testified.

Observations of Pressure every minute.

Experiment I.	Experiment II.
Temp^e at commencement, 97° F. Pressure in lbs.	Temp^e at commencement, 126° F. Pressure in lbs.
1·0	0·
2·9	2·8
4·4	5·9
6·0	8·8
7·7	12·6
9·05	16·1
11·	20·8
12·35	26·1
13·85	31·8
15·1	38·
16·8	44·
20·1	49·9
24·8	54·8
31·	59·4
37·2	63·8
44·2	Temperature at conclusion, 139° F.
51·4	
58·2	
63·	
Temperature at conclusion, 126° F.	

III.—*Experiments on the Total Heat of Steam.*

By J. P. Joule, LL.D., F.R.S., *&c.*

Read November 29th, 1859.

The total heat of steam is understood to mean that which is evolved when the steam is condensed into water of the freezing temperature. It is a mixed quantity, and consists of: 1st, the heat due to the change of state from vapour to water, or the true latent heat; 2nd (as I showed long ago)* the heat arising from the work done on the vapour in the act of condensation; and 3rd, the heat evolved by the water during its descent from the temperature of condensation to the fixed temperature chosen, viz., 0° centigrade.

The importance of a correct determination of the total heat can hardly be over estimated, and it is fortunate that one of the most eminent physicists of modern times has made it the object of long and elaborate research. It is not my design to attempt to improve upon the experiments of M. Regnault, but having had the opportunity of making some determinations in a different manner from that he employed, I think my results may not be thought without interest.

In Regnault's experiments the steam was passed into globes and a worm immersed in the water of a calorimeter. By the use of an artificial atmosphere connected

* *Transactions of British Association*, Birmingham, 1849.

with the worm, the operator was enabled in all cases to operate under similar circumstances as to the relative pressures of the steam and atmosphere.

In my own experiments, a vulcanized india-rubber tube, eight inches long, was attached to the nozzle of a short pipe (furnished with a stop-cock), connected with the top of an upright boiler. To the end of the india-rubber tube a brass nozzle was attached. The stop-cock was left constantly open. In making an experiment the brass nozzle through which the steam was blowing was suddenly plunged into a can of water and then, after two or three minutes, suddenly removed again. The weight gained by the can indicated the quantity of water condensed, which, with the observations of temperature before and after the experiment, afforded the means of computing the total heat of the steam.

The requisite corrections were readily made and not of large amount. They arose from the heat lost by the steam by conduction in passing from the boiler to the can, the thermal effects of the atmosphere on the can itself, and the evaporation of the water from the can which took place before the weighing was accomplished. The data for these were derived from observations made after each experiment. The following table comprises the results I obtained:

Time during which the steam was introduced, in minutes.	Weight of water in can, including can reduced to sp. heat of water. W.	Temperature of water. Before experiment. t.	After experiment. t'.	Weight of steam condensed in grains. w.	Total pressure of steam in inches of mercury.		Total heat of steam. $\frac{W(t'-t)}{w}+t'$		Regnault's result.
2	140351	6·362°	43·443°	8700	40·0		641·64		
$2\frac{1}{2}$	140351	6·587°	56·936°	12089	36·4		641·48		
$2\frac{1}{2}$	140351	6·457°	58·785°	12577	36·95	37·25	642·73	638·43	638·77
2	140351	4·893°	43·592°	9205	38·25		633·61		
$2\frac{1}{2}$	140351	5·096°	53·909°	11686	36·1		640·16		
2	140351	5·139°	52·814°	11574	35·8		630·94		
2	140351	6·384°	48·228°	9835	57·3		645·37		
2	140351	6·54°	58·325°	12418	57·6		643·61		
3	140351	6·399°	55·083°	11609	52·2	57·52	643·66	644·77	642·87
2	140351	6·54°	48·048°	9775	54·6		644·03		
2	140351	5·096°	58·347°	12800	60·3		642·24		
2	140351	5·371°	56·523°	12103	63·1		649·70		
2	140351	6·529°	63·043°	13490	105·1		651·02		
2	140351	6·684°	58·835°	12272	117·37		655·27		
2	140351	6·684°	55·285°	11276	115·3	111·58	660·21	655·45	649·6
2	140351	5·342°	53·257°	11191	109·2		654·18		
2	140351	5·574°	54·033°	11266	112·6		657·83		
2	140351	5·516°	64·891°	14141	109·9		654·2		

In the above experiments the steam was condensed at twice the rapidity it was in those of Regnault. I had also an advantage in the size of my boiler, which was eight feet high by two feet ten inches in diameter, whereas his was only two feet seven inches by two feet one inch in diameter. Owing, however, to the small number of my experiments, the results at which I have arrived can only be regarded as confirmatory of those of the French physicist. I believe, nevertheless, that the simple method I have adopted may be resorted to with advantage whenever it shall be required to obtain a further increase of accuracy.

IV.—*Experiments on the Passage of Air through Pipes, and Apertures in thin plates.*

By J. P. Joule, LL.D., F.R.S., *&c.*

Read April 3rd, 1860.

Sir Isaac Newton, Polenus, Daniel Bernoulli and others have observed that water, when it is made to flow out of a vessel through a hole cut in a thin plate, becomes contracted in diameter and increased in velocity at a short distance from the hole, the ratio of the diameter of the stream at its narrowest part to the diameter of the hole being, according to Newton's experiments, as twenty-one to twenty-five. The phenomenon is occasioned by a concourse of the particles of water as they enter the orifice, and may, as Venturi has shown, be obviated by employing a short pipe instead of a hole in a thin plate.

Air, and other fluids are known to comport themselves in the same manner as water. The subject is one of considerable importance, and as I have had an opportunity of trying some experiments on it, I trust they will be found of sufficient interest to warrant my offering them to the notice of the Society.

The principal part of my apparatus was a large organ-bellows, which, by means of weights laid on the top could be worked at pressures varying from 1·44 to 5·65 inches of water as indicated by the difference of level of water in a bent glass tube. A circular hole, two and a half

inches in diameter, was cut in the chest, and in this could be placed, or to it affixed, thin plates with holes, or other means for the egress of air.

The method of experimenting was to note the time in seconds and tenths occupied by the running down of the bellows. The capacity of the bellows being known to be 29,660 cubical inches, this observation gave the quantity of air issuing per second, plus the unavoidable leakage of the bellows. The amount of the latter was ascertained by observing the time in which the bellows ran down when the hole was made tight, and being subtracted from the gross effect, gave the quantity which actually passed through the orifice.

Theoretically the quantity of air emitted in a given time ought to be proportional to the size of the aperture in the thin plate, multiplied by the square root of the pressure, or, in other words, the quantity emitted per square inch of aperture divided by the square root of the pressure ought to be a constant quantity. My observations to confirm this law were made with circular holes in thin tinned iron, measuring 0·535, 1·045, and 1·61 inch in diameter respectively. The pressure and temperature of the air in all the experiments were about 29·8 in. and 4° cent.

Diameter of aperture.	Pressure, in inches of water.	Cubic inches of Air discharged per second, reduced to one square inch aperture.	Cubic inches of Air discharged per second, divided by square root of pressure.
0·535	1·44	496	413·3
	5·6	1033·3	436·8
1·045	1·44	541·4	451·2
	5·6	1058·4	447·5
1·61	1·44	589·5	491·2
	5·6	1132·7	478·7

The last column of the above table shows the accuracy of the law so far as pressure is concerned, but seems to indicate a slight increase of the quantity issuing per square inch as the aperture becomes larger.

The law is also verified so far as pressure is concerned by the following tabulated experiments with tubes of various lengths and diameters, but all terminated by a short piece of wide pipe, three inches long by two and a half inches in diameter, which was inserted into the bellows :—

Length & diameter of tubes.	Pressure.	Cubic inches of Air discharged per second per square inch aperture.	Cubic inches of Air discharged per second per square inch aperture divided by square root of pressure.
44 and 0·875	1·44	562·9	469·1
	3·52	909·7	484·7
20 and 0·98	1·44	671·5	559·6
	3·52	1049·4	559·1
20 and 1·594	1·44	710·6	592·2
	3·52	1117·1	595·1

At an early period of the research it was found that a very slight bur or projection on the edge of the hole in a thin plate produced a remarkable change in the quantity of effluent air. I had holes of the respective least diameters, 0·535, 0·75, and 1·61 inch, cut out of a thin plate of tinned iron by a brace-bit. A slight bur projected to one-fortieth of an inch beyond the plain surface. The following experiments were then made, using a pressure of air equal to 1·44 inch of water :—

Position of the bur with respect to the bellows.	Cubic inches of Air per second, reduced to one square inch of aperture.		
	Hole of diameter 0·535 inch.	Hole of diameter 0·75 inch.	Hole of diameter 1·61 inch.
Outwards	597·8	579·2	647·4
Inwards	529·3	524·4	584·7

A hole one inch square, without any bur, gave 567 cubic inches per second.

The influence of a tube in increasing the quantity of effluent air has been already adverted to. It was a matter of considerable interest to determine the length of tube which would produce the maximum effect. In my first experiments to determine this point, I employed a tube

0·98 of an inch diameter, and terminated at one end by a piece of wider tube, three inches long and two and a half inches in diameter—

Length of tube of 0·98 inch diameter.	Cubic inches of air per second, reduced to square inch of aperture of narrow tube.	
	Air entering by the short length of wide tube.	Air entering by the narrow tube.
40 inches.	642·7	
20 ,,	666·7	
10 ,,	714·2	
4 ,,	759·4	728
2 ,,	787·7	723·3
1 ,,	806·5	730·4
½ ,,	810·9	646·5
¼ ,,	803·7	578 flapping sound.
3/16 ,,	749·5 flapping sound.	546
⅛ ,,	685·5	547·5
1/16 ,,	666·6	541·4

In an experiment in which a flange with a hole of one and a quarter inch diameter in its centre was placed on the wide end, the quantity of air entering by the narrow tube reduced to the length of three-sixteenths of an inch was increased from 546 to 600.

The next experiments were made with a tube, 0·92 of an inch outside and 0·8 inside diameter, successively reduced in length. The inner sharp edge was removed at one end, and the outer edge at the other end of the tube. It will be seen that the greatest quantity of air flowed when it entered at the end from which the inner edge had been removed.

Length of tube of 0·8 inch diameter.	Cubic inches per second, per square inch of aperture.	
	Air entering the end from which the inner sharp edge was removed.	Air entering the end from which the outer sharp edge was removed.
44 inches.	513·5	473·2
24 ,,	564	538
12 ,,	589·6	573·4
4 ,,	660	637·4
2 ,,	685·2	668
1 ,,	726·2	663·2
½ ,,	699·6	594
¼ ,,	594·8	526

My last experiment was with a hollow cone, the sides of which formed an angle of 60°, and the opening at one end was three inches, and at the other 0·625 in diameter. Using a pressure of 1·44, the quantities of effluent air per second per square inch of narrowest aperture were, accordingly as the air entered at the broad or narrow apertures, 666·1 and 510·7 respectively.

The height of a column of air of the density and temperature of that used in the experiments, which would give a pressure of 1·44 inches of water, is 88·93 feet. The formula for very small pressures is $v = \sqrt{2gh}$. Thus the theoretical velocity in the absence of disturbing causes would be 75·64 feet per second, which gives 907·7 cubic inches issuing per second through an orifice one inch square. Calling this theoretical efflux unity, the above experiments give—

For apertures in thin plates - - - - -	·6074
For a tube of the same diameter as length -	·7676
For a similar tube with a wide entrance tube	·8933

I have not been able to detect any effect due to vibration of the issuing stream. By placing the end of a tube composed of thin metal, four feet long and one inch diameter, at about half an inch distance from an aperture in a thin plate of one inch diameter, musical tones were produced, which by increasing the pressure gradually, ascended in harmonics through a scale of many octaves. The same musical effects could be produced, using a constant pressure of air, by moving the tube nearer the aperture through the space of a tenth of an inch. Savart and Masson have adduced facts of this kind to prove that air rushing out of an aperture has a vibratory motion. Although I do not admit this conclusion, there can be no doubt that the vibration constituting sound, produced by such methods as above indicated, will be able to travel

back to the air rushing through the orifice if its velocity be not greater than 1090 feet per second. I have failed, however, to discover any sensible influence from this cause on the velocity of efflux. It occurred to me also to try whether the air issued with a rotary motion; but such experiments as I have been able to make with vanes have led me to no decisive conclusion, although there can be no doubt that many circumstances might cause such vortices, the operation of which would be to diminish the velocity of efflux.

V.—*Supplementary Researches in the Higher Algebra.*

By JAMES COCKLE, M.A., F.R.A.S., F.C.P.S., *Barrister-at-Law.*

Read November 29th, 1859.

§ 33.

I AM desirous of submitting to the Society a Supplement to the "Researches" which I have had the honour of laying before it, and which appeared in the last volume of its *Memoirs*. And, first, the relations

$$a+d=5,\ a^2+d^2=15,\ b^2+c^2=10,$$
$$ab+cd=-(ac+bd)=5,\ \phi(i)-\phi(i^2)=\rho$$

may conveniently be added to those given in § 2, and it will also be convenient to establish the notation

$$\tau_1(u^m)=\Sigma' v^m w^m,\ \ \tau_2(u^m)=\Sigma' v^m x^m.$$

§ 34.

We may pass in two ways from $v^2(wz+xy)$ to U_r, and either cyclically or by forming (epimetric) functions into which each square and product enters once and once only, and which contain no cube or higher power. In the former case the operand is subjected to the processes which Mr. Harley denotes by Σ'_1 and Σ'_3: in the latter, using the notation of the theory of interchanges,* we are led by Σ' to U_1 and $U_1(\overset{wz}{\cdot\cdot})$. And we may write

* On this theory see Mr. Jerrard's Essay, &c.; his Reflections, &c. (*Phil. Mag.* June 1845); my Observations, &c. (*Ibid.* May 1857); &c.

$$\Sigma'_1 = \mathrm{U}_1,\ \Sigma'_3 = \Sigma'' = \mathrm{U}_1\binom{wz}{\cdot} = \mathrm{U}_1\binom{xy}{\cdot\ \cdot}$$

where Σ'' corresponds to the cycle $vzxyw$. Under the cyclical aspect Σ' is singularly efficient as an instrument of calculation, and a transformation, to which I was conducted some time since, enables us to demonstrate with great ease and brevity a proposition asserted in § 28. Mr. Harley's notation (which appears in the same volume of the *Memoirs*) at once enables us to write

$$f^m(i)f^n(i^4) = \Sigma v^{m+n} + i\Sigma' v^m z^n + i^2\Sigma' v^m y^n + i^3\Sigma' v^m x^n + i^4\Sigma' v^m w^n$$
$$= \Sigma' v^m f^n(i^4) = \Sigma' v^n f^m(i).$$

Consequently

$$\mathrm{I} = \Sigma' v^m\{f^n(i^4) + f^n(i)\} = \Sigma' v^n\{f^m(i) + f^m(i^4)\},$$
$$\mathrm{J} = \Sigma' v^m\{f^n(i^3) + f^n(i^2)\} = \Sigma' v^n\{f^m(i^2) + f^m(i^3)\},$$

and $\mathrm{I}+\mathrm{J}$ and IJ are invariable under interchanges of m and n and powers of i and, inasmuch as the latter is not symmetric, $\mathrm{I}^2+\mathrm{J}^2$, and, therefore, (Σ) are functions of τ.

§ 35.

We may, no doubt, obtain relations connecting epimetrics of various forms, and indeed I have actually obtained them (*vid. Phil. Mag.* February 1854 and August 1856). But the following results, obtained with the greatest ease, show the practical superiority of the cyclical process:

$$(\Sigma' vw)^2 = \Sigma' vw \,.\, \Sigma' vw = \Sigma'(v^2w^2 + 2v^2wz)$$

or, $\{\tau(u)\}^2 = \tau(u^2) + 2\Sigma' v^2wz$; . . . . (20)

$$\Sigma' v\Sigma' v^2(w+z) = \Sigma' v^3(w+z) + 2\tau(u^2) + \Sigma' v^2(wz - xy)$$
$$= \Sigma' v^3(w+z) + 2\tau(u^2) - \{\tau(u)\}^2 + 2\Sigma' v^2wz,$$

or, $\{\tau(u)\}^2 = \Sigma' v^3(w+z) + 2\tau(u^2) + 2\Sigma' v^2wz$; . . . (21)

consequently, combining (20) and (21), we find

$$\Sigma' v^3(w+z) + \tau(u^2) = 0 \quad . . . . (22)$$

These results, which have reference to the trinomial quintic but may readily be generalised, facilitate a transformation now to be discussed.

§ 36.

Let $u'=pu+u^3$, $a_n=f(i^n)$ and $b_n=f^3(i^n)$, then $\theta(u')=\theta(pu+u^3)$

$$=(a_1p+b_1)(a_2p+b_2)(a_3p+b_3)(a_4p+b_4)$$
$$=\mathrm{A}p^4+\mathrm{B}p^3+\mathrm{C}p^2+\mathrm{D}p+\mathrm{E}, \text{ say,}$$

and $\mathrm{A}=a_1a_2a_3a_4=\theta(u)$, $\mathrm{E}=b_1b_2b_3b_4=\theta(u^3)$,

$$\mathrm{B}=a_1a_4(a_2b_3+b_2a_3)+a_2a_3(a_1b_4+b_1a_4)$$
$$=\rho\tau(u)\mathrm{J}-\rho\tau(u)\mathrm{I}=-2\rho^2\tau(u)\Sigma' v^3(w+z)$$
$$=2\,.\,5\tau(u)\tau(u^2),$$

in virtue of (22) and of the relation $\rho^2=5$.

Again, $\mathrm{C}=a_1a_4b_2b_3+a_2a_3b_1b_4+(a_1b_4+b_1a_4)(a_2b_3+b_2a_3)$

$$=\rho\tau(u)(b_2b_3-b_1b_4)+\mathrm{IJ}=\rho\tau(u)(b_2b_3-b_1b_4)+\theta(u^2)$$
$$=\theta(u^2)-5\tau(u)\{2\tau(u^3)-\Sigma v^3w^3\}; \text{ and}$$
$$\mathrm{D}=b_1b_4(a_2b_3+b_3a_2)+b_2b_3(a_1b_4+b_4a_1)$$
$$=b_1b_4\mathrm{J}+b_2b_3\mathrm{I}=5\tau(u^2)\{2\tau(u^3)-\Sigma v^3w^3\}.$$

These results furnish us with the development of $\theta(u')$ and the only observations* which it seems necessary to make upon them are that the formulæ of § 34 give

$$\mathrm{I}=-\mathrm{J}=\Sigma' v^3(w+z),$$

and that from

$$b_2b_3=(i^2+i^3)\Sigma' v^3(w^3+z^3)+(i+i^4)\Sigma' v^3(x^3+y^3)$$
$$=-\rho\tau(u^3)+(i+i^4)\Sigma v^3w^3$$

we pass at once to

$$b_2b_3-b_1b_4=-\rho\{2\tau(u^3)-\Sigma v^3w^3\}.$$

§ 37.

I now proceed to other points which appear to me to place in a clearer light the relation in which these researches stand to the theory of quintics. Let a, b, c and d be any four symbols, and X a rational function of them. The most general substitution that can be applied to X is one consisting of three successive binary interchanges, and either side of the equivalence

* See also my Observations, &c., *Phil. Mag.* May 1859. The transformed equation is there exhibited.

$$X\binom{ab}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}\binom{ad}{\cdot\,\cdot}=Xl$$

may be taken to represent such a substitution. Let us make

$$X_1=\phi(a,\ b,\ c,\ d)$$
$$X_2=\phi(b,\ d,\ a,\ c)=Xl$$
$$X_3=\phi(c,\ a,\ d,\ b)=Xl^3$$
$$X_4=\phi(d,\ c,\ b,\ a)=Xl^2.$$

§ 38.

Next, let θ be any symmetric function of X. The substitution l^r or its equivalent l^{4m+r} (m an integer) has, as we see, no effect upon θ. Therefore

$$\theta=\theta l=\theta l^2=\theta l^3.$$

§ 39.

From $\theta=\theta l$ and the transformations afforded by the theory of interchanges we find

$$\theta\binom{cd}{\cdot\,\cdot}=\theta\binom{ab}{\cdot\,\cdot}\binom{ac}{\cdot\,\cdot}=\theta\binom{ac}{\cdot\,\cdot}\binom{bc}{\cdot\,\cdot}=\theta\binom{bc}{\cdot\,\cdot}\binom{ab}{\cdot\,\cdot}$$
$$\theta\binom{bd}{\cdot\,\cdot}=\theta\binom{ac}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}=\theta\binom{ad}{\cdot\,\cdot}\binom{ac}{\cdot\,\cdot}=\theta\binom{cd}{\cdot\,\cdot}\binom{ad}{\cdot\,\cdot}$$
$$\theta\binom{ac}{\cdot\,\cdot}=\theta\binom{ab}{\cdot\,\cdot}\binom{ad}{\cdot\,\cdot}=\theta\binom{ad}{\cdot\,\cdot}\binom{bd}{\cdot\,\cdot}=\theta\binom{bd}{\cdot\,\cdot}\binom{ab}{\cdot\,\cdot}$$
$$\theta\binom{ab}{\cdot\,\cdot}=\theta\binom{bc}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}=\theta\binom{cd}{\cdot\,\cdot}\binom{bd}{\cdot\,\cdot}=\theta\binom{bd}{\cdot\,\cdot}\binom{bc}{\cdot\,\cdot}.$$

§ 40.

From $\theta=\theta l^3$ and the transformations afforded by the theory of interchanges, we find

$$\theta\binom{bd}{\cdot\,\cdot}=\theta\binom{ac}{\cdot\,\cdot}\binom{ab}{\cdot\,\cdot}=\theta\binom{bc}{\cdot\,\cdot}\binom{ac}{\cdot\,\cdot}=\theta\binom{ab}{\cdot\,\cdot}\binom{bc}{\cdot\,\cdot}$$
$$\theta\binom{ab}{\cdot\,\cdot}=\theta\binom{cd}{\cdot\,\cdot}\binom{ac}{\cdot\,\cdot}=\theta\binom{ac}{\cdot\,\cdot}\binom{ad}{\cdot\,\cdot}=\theta\binom{ad}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}$$
$$\theta\binom{cd}{\cdot\,\cdot}=\theta\binom{ad}{\cdot\,\cdot}\binom{ab}{\cdot\,\cdot}=\theta\binom{bd}{\cdot\,\cdot}\binom{ad}{\cdot\,\cdot}=\theta\binom{ab}{\cdot\,\cdot}\binom{bd}{\cdot\,\cdot}$$
$$\theta\binom{ac}{\cdot\,\cdot}=\theta\binom{cd}{\cdot\,\cdot}\binom{bc}{\cdot\,\cdot}=\theta\binom{bd}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}=\theta\binom{bc}{\cdot\,\cdot}\binom{bd}{\cdot\,\cdot}.$$

§ 41.

From $\theta=\theta l^2$ adjoined to $\theta=\theta l$ we obtain

$$\theta\binom{ad}{\cdot\,\cdot}=\theta\binom{bc}{\cdot\,\cdot}=\theta\binom{ab}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}=\theta\binom{ac}{\cdot\,\cdot}\binom{bd}{\cdot\,\cdot},$$

and all the interchanges are now reduced to binary ones, two of which are equivalent one to the other. Consequently θ may be represented as the root of a sextic

$$\theta^6+D_1\theta^5+D_2\theta^4+\cdot\cdot+D_6=0,$$

whereof the coefficients are symmetric functions of a, b, c and d.

§ 42.

The conjugate interchanges now play an important part. To the equation

$$\xi=R\{\theta,\ \theta\binom{ad}{\cdot\,\cdot}\},$$

where R denotes a rational and symmetric function, apply every possible interchange. The result will be known by examining the effect of the binary changes alone, and we find

$$\begin{aligned}
&\xi\binom{ab}{\cdot\,\cdot}=R\{\theta\binom{ab}{\cdot\,\cdot},\ \theta\binom{ad}{\cdot\,\cdot}\binom{ab}{\cdot\,\cdot}\}=R\{\theta\binom{ab}{\cdot\,\cdot},\ \theta\binom{cd}{\cdot\,\cdot}\}\\
&\xi\binom{ac}{\cdot\,\cdot}=R\{\theta\binom{ac}{\cdot\,\cdot},\ \theta\binom{ad}{\cdot\,\cdot}\binom{ac}{\cdot\,\cdot}\}=R\{\theta\binom{ac}{\cdot\,\cdot},\ \theta\binom{bd}{\cdot\,\cdot}\}\\
&\xi\binom{ad}{\cdot\,\cdot}=\xi\\
&\xi\binom{bc}{\cdot\,\cdot}=\xi\\
&\xi\binom{bd}{\cdot\,\cdot}=R\{\theta\binom{bd}{\cdot\,\cdot},\ \theta\binom{ad}{\cdot\,\cdot}\binom{bd}{\cdot\,\cdot}\}=R\{\theta\binom{bd}{\cdot\,\cdot},\ \theta\binom{ac}{\cdot\,\cdot}\}\\
&\xi\binom{cd}{\cdot\,\cdot}=R\{\theta\binom{cd}{\cdot\,\cdot},\ \theta\binom{ad}{\cdot\,\cdot}\binom{cd}{\cdot\,\cdot}\}=R\{\theta\binom{cd}{\cdot\,\cdot},\ \theta\binom{ab}{\cdot\,\cdot}\},
\end{aligned}$$

consequently ξ cannot receive, by permutation of a, b, c and d more than the three values

$$R\{\theta,\theta\binom{ad}{\cdot\,\cdot}\},\ \ R\{\theta\binom{ab}{\cdot\,\cdot},\theta\binom{cd}{\cdot\,\cdot}\},\ \ R\{\theta\binom{ac}{\cdot\,\cdot},\theta\binom{bd}{\cdot\,\cdot}\}$$

which we may call ξ_1, ξ_2, and ξ_3 respectively, and which are the roots of a cubic that may be written

$$\xi^3+\beta\xi^2-\gamma\xi+\delta=0,$$

and whereof the coefficients are symmetric in a, b, c and d.

§ 43.

Write θ_1 in place of θ, and let

$$\theta_2=\theta\binom{ab}{\cdot\,\cdot},\ \ \theta_3=\theta\binom{ac}{\cdot\,\cdot},\ \ \theta_4=\theta\binom{ad}{\cdot\,\cdot},$$
$$\theta_5=\theta\binom{bd}{\cdot\,\cdot},\ \ \theta_6=\theta\binom{cd}{\cdot\,\cdot};$$

the foregoing discussion shows that if

$$\xi_1=\theta_1+\theta_4,\ \ \xi_2=\theta_2+\theta_6,\ \ \xi_3=\theta_3+\theta_5,$$

then

$$\Sigma\xi_1\xi_2=\Sigma\theta_1\theta_2-(\theta_1\theta_4+\theta_2\theta_6+\theta_3\theta_5),\ \text{or}$$
$$\gamma=\theta_1\theta_4+\theta_2\theta_6+\theta_3\theta_5-D_2,$$

and γ is symmetric in a, b, c and d.

§ 44.

Now there are two conditions essential to the application of these results to the theory of quintics:

1°. The symbol γ must be the root of an equation with known coefficients.

2°. The symbols D must be known.

§ 45.

The first will be attained if we assume

$$X_1 = x_1 + ax_2 + bx_3 + cx_4 + dx_5,$$

for, the interchanges of x_2, x_3, x_4 and x_5 being equivalent to corresponding interchanges of *a, b, c* and *d*, the expression γ will be symmetric with respect to those four roots and, therefore, a rational function of the remaining root x_1, and the root of a determinate equation of the fifth degree.

§ 46.

It will be remembered that θ is symmetric in X, but that all is otherwise arbitrary. Hence, in order to attain the second condition, we may give to θ any form consistent with that symmetry. One mode of seeking to attain it is by giving a maximum of symmetry to θ with respect to x, in other words, by endeavouring to construct a SYMMETRIC PRODUCT. We are thus led to the sextic in θ.

§ 47.

These results justify the form which Mr. Harley gave to the factors of the resolvent product, and by which he has so greatly simplified my discussion (compare *Phil. Mag.* December 1852 and March 1853 with his Memoir). Viewed in the light of the present Memoir, the Method of Symmetric Products has (beyond invoking the aid of interchanges) no special relation to other methods in the theory of equations. If we suppose the resolvent product

to vanish it may be regarded as a modification and simplification of the theory of Lagrange and Vandermonde; but it is needless to assume this evanescence, and indeed by connecting it with Euler's process far more striking formulæ are obtained. Considered as a process of elimination I shall give it a discussion elsewhere.*

* See a paper "On Equations of the Fifth Degree," which will appear in the forthcoming Diary.

VI.—*Remarks on the Australian Gold Fields.*
By W. S. Jevons.

Read November 15th, 1859.

I. The Geological Characteristics of the principal Gold Districts.

To prepare the way for some general conclusions as to the geological source of gold, I shall commence with brief descriptions of those more important gold-producing localities which I lately enjoyed an opportunity of visiting.

Bendigo.—The celebrated Bendigo consists of a long shallow valley—say ten miles long, and from one to three miles wide. On both sides it is distinctly bounded by hilly ranges of moderate and rather uniform height, from which spur-ranges often advance towards the middle of the valley, while corresponding *gullies** or smaller valleys run up a short distance into the surrounding country. The ranges are entirely composed of a rather soft schistose or slaty rock, varying in colour from red to yellow, and decidedly belonging to the Silurian era, but the strata are cloven, here and there, by remarkable walls or dykes of quartz rock, usually called *reefs,* which always run in a nearly north and south direction, independently of the form of the ground. The quartz is often of a pure milk-white colour, but also, in parts, contains considerable

* *Gully* is an universal term in Australia for any *hollow*, whether it be a precipitous ravine of great size or a trifling inequality of the ground.

quantities of the sulphides of iron, arsenic and copper, which are known by the common name of *mundic*, and are a pretty sure indication that gold is present in a proportion highly profitable to the miner. At the same time, these sulphides prove a great obstacle to the process of amalgamation, causing a serious loss both of mercury and of gold.

The lower parts of the valley are partially filled up with detritus which has evidently been washed down by streams of water from the neighbouring hills. Such detritus consists of red and white mottled clays, more or less mixed with sand and gravel, among which quartz pebbles are conspicuous.

The deposits of gold dust which gave to Bendigo its reputation lie, or rather used to lie, principally in the very lowest strata of the alluvial detritus (technically called by geologists the *auriferous drift*), but are especially accumulated in the deepest channels or hollows worn into the schistose bottom rocks at some former time. Thus, while a large area of ground throughout the valley has proved auriferous, the most valuable claims usually lie in a broad line which does not often coincide with the present Bendigo Creek, or line of drainage. This line or lead of rich gold deposits has more lately been traced two or three miles below the mouth of the valley, where the present surface of the ground is nearly level.

Forest Creek, which lies close to the new and considerable town of Castlemain, is another large, rich and well known digging. In general nature it so much resembles Bendigo that no separate description is necessary; the schistose ranges, however, are here more bold, and assume a characteristic shape, resembling a very obtuse pyramid. I am not aware that quartz reefs have yet been opened here.

Creswick's Creek, formerly of high reputation, and a

host of smaller gold-bearing localities may be described almost in the same terms — as valleys or gullies among low schistose hills. But not uncommonly the auriferous drift assumes so level an upper surface as to be denominated a *flat*.

BALLAARAT. — I come now to the celebrated Ballaarat, the great richness and still more the complicated and remarkable structure of which render it by far the most interesting of the Australian gold fields. We shall be greatly assisted, too, in understanding its geological formation by the excellent geological map produced under the direction of Mr. Selwyn, government geologist of Victoria.

From this map it will be seen that Ballaarat consists of a somewhat basin-shaped accumulation of alluvium, or auriferous drift, surrounded by detached and bold hills of schistose or slaty rock, between which run various gullies filled by extensions of the alluvium. All the inequalities of the surface and the continuity of the depressions may be clearly traced by the contour lines. A very large part of the total area of alluvium is distinctly auriferous, and a large part indeed has been turned up or undermined by the gold diggers in their eager search. But on the map only those spots of ground are marked as *gold workings* where the deposit was especially rich; now it has been found and is shown that these richest deposits occur in broad continuous lines or bands called leads of gold, which occupy the deepest depressions of the schistose bottom rock. The leads are all branches of one main lead or body; and while the elevation of the bottom of this main lead is about one thousand two hundred feet above the sea, the branch leads universally show a more or less gradual rise of level. It may be observed, too, that the leads show scarcely any coincidence with the present gullies or water-courses, and do not bear a very distinct relation even to the position of the present schistose hills.

A fresh feature is now to be noticed which, although unconnected geologically with the gold, is of some importance to our subject, because it often conceals the golden strata and obstructs the miner in reaching them. I allude to a great sheet of basalt or, in fact, lava, which at a very recent geological period has overflowed the beds of auriferous drift often to a thickness of fifty feet. At Ballaarat this overflow has only partially taken place, and the viscid current of lava appears to have been stayed in its progress so as to form a plain bounded by a steep slope. A large and irregular area of country in Victoria, perhaps one hundred miles in length, is covered by this basaltic rock. Melbourne stands upon the edge of the basalt, and its houses and stores are built of this hard blue vesicular rock. Standing upon the higher parts of that city, the view extends inland over an immense sheet of lava often nearly as level as the sea, and only covered by a thin layer of fine brown soil.

Extinct volcanoes also, unmistakeable in form and nature, occur in several parts of this basaltic country. Thus Mount Buninyong, lying seven miles east of Ballaarat, is an obtuse cone of basalt, becoming more and more scoriaceous towards the summit, whence I brought several specimens of decided scoria taken from a slight depression which was, undoubtedly, an imperfect crater. From this mountain a very extensive view was obtained over a country generally flat. A mile or two to the north was seen Mount Warrenheip, the counterpart of Buninyong, and, like it, an ancient volcano; while Mount Blackwood, Mount Clarke, and other eminences lying at various distances and points of the compass, could also be distinguished as of similar origin by their isolated conical or rounded form.

Again, twenty or thirty miles to the north of Ballaarat there stand, scattered over the basaltic plain, a numerons group of very smooth-rounded eminences known as the

Bald Hills, or Hills of Lava. They are of basaltic rock, which, it is my impression, is not more vesicular or scoriaceous than in other parts of the country; thus these hills differ from volcanoes proper. It is no easy matter to conceive the cause of these singular Bald Hills, or the source of the immense body of lava which now forms the surface of a large part of Victoria, and must once have been a liquid mass, glowing in the open air. Among the various ancient rocky strata of the globe, it is well known that overflows and protrusions of volcanic rock are often met with, while I am not aware that an ancient (*i.e.* primary, secondary, or tertiary) volcano, embedded below more recent strata, has ever been recognised. Is it possible that in Victoria we find the type of the ancient volcanic eruption in which lava was all abundant, while modern volcanoes with lofty cones are distinguished by the discharge of much gaseous matter?

But, to return from this digression: for many years — indeed nearly up to the time of the geological survey of Ballaarat — the gold miners had no thought that rich deposits of gold lay concealed beneath the basaltic overflow; but greater experience, combined with increased means of penetrating the hard rock and of sinking deep and expensive shafts, enabled them to trace out several fine leads of gold, extending quite under the new town of Ballaarat up to the lake or swamp indicated on the plan. Numerous companies of co-operative miners have lately taken claims on these leads and, with the aid of steam engines, pumps and other machinery, are raising and washing great quantities of gold. I descended one of these mines (on the Malakoff lead), being lowered by steam power down a well-constructed shaft to a depth of three hundred feet. From the bottom of the shaft a gallery had been driven to reach the lead of gold, which was then followed to the limits of the claim.

The lead was here contained in a deep, well-marked channel in the schistose bottom rocks, now filled up by a most peculiar alluvium, consisting of quartz gravel, fine black clay, and an abundance of lignite or blackened fossil wood,—complete trunks of trees, indeed, being sometimes encountered by the miners. The gold dust lay most thickly in the very lowest part of the channel, where it was not difficult to detect the shining grains and pick them out with the fingers; they were partly entangled in the crevices and joints of the bottom rock, but smaller quantities of gold dust lay on the sides of the channel, or were disseminated through the alluvium.

We need only see such a mine as this to be convinced that these singular leads of gold lie in the water-courses, or river-beds of a former age, where streamlets and rivers ran down from hills now washed away. The history of alluvial gold can here be read as plainly as in a book; great masses, perhaps mountains, of schistose rock have in the course of ages become disintegrated; the quartz reefs which the hills contained were then broken down and filled the river bed with smoothly worn gravel, while the particles or nuggets of gold thus liberated from their quartzose matrix sought, by means of their high specific gravity, the lowest possible position, and there accumulated. Nature has in this way for many ages been performing the self same operations to which the gold miner has recourse in extracting and separating his gold.

A large space of time indeed must have elapsed since the lower parts of the alluvium and the large quantity of gold which they contain were deposited, because we find them covered by two or three hundred feet depth of other alluvium since accumulated, not to speak of the basaltic layer, which proves that extensive volcanic eruptions have both commenced and become quiescent in the intervening period.

It will be noticed that a number of quartz reefs, known to be auriferous, are marked upon the Government Map from actual survey, their parallel and meridional direction being strikingly shown. Many other reefs however exist, some perhaps unauriferous, but mostly untested or imperfectly tested for gold.

Western Gold Fields of N. S. Wales.— In the western district of New South Wales, where, it will be remembered, Mr. Hargreaves made the first *practical* discovery of Australian gold, the prevailing strata are of a hard slate rock belonging to the Silurian era. Quartz reefs are found abundantly penetrating the strata. The country, accordingly, is of a more rugged and mountainous character than in Victoria, rapid streams or torrents flowing from among the steep stony ranges. The auriferous alluvium generally consists of gravel mixed with a little clay, lying in or close to the present water-course; but there exist many places where the stream has in the course of time worn for itself a new channel, and left in its former place large accumulations, even hills, of auriferous drift. Making allowance for the harder nature of the prevailing rock, the same geological principles prevail in these diggings as in those above mentioned.

Maryborough. — This small town is situated in the midst of many gold-producing localities of inferior importance, but I have only to notice here a small hill, close to the town, consisting of schistose rock penetrated by a considerable quartz reef. A mass of feldspathic porphyry is also to be found close to the reef, and sometimes penetrated by leaders or thin veins of quartz in a very curious manner.

Ovens Gold District. — In this district are comprised many separate localities, such as Jackandandah, Woolshed, Indigo, &c.; but for my present purpose, I need only describe those auriferous flats or gullies lying within a few

miles of Beechworth, the principal town, which is situated upon the May-day Hills, one thousand seven hundred feet above the sea. The diggings in question lie considerably above the level of the surrounding country, and we can look for the source of the contained gold only to the highest parts of the hills. Now the May-day range is composed of granite, and no stratified rocks were to be seen; so that it is surprising, and almost unaccountable, to find among the auriferous alluvium clay containing an abundance of white quartz gravel, evidently the detritus of schistose hills penetrated by auriferous reefs. We are driven to suppose that Silurian strata once covered the granitic range, but have since been entirely disintegrated and removed.

Mount Tarrengower. — Here we meet very different geological features. The gold produce is derived from several very fine and rich quartz reefs which traverse a bold mountain range, principally if not entirely composed of Plutonic rocks. Unimportant alluvial diggings lie at the eastern base of the mount. I descended and examined two of the quartz gold claims or mines, mere rude narrow spaces, where part of the dyke of quartz had been abstracted from between the walls of the containing rocks. In these cases, the reef did not crop out at the surface, but shafts had been sunk to a depth of fifty or sixty feet in order to strike upon it.

The foundations of Mount Tarrengower are undoubtedly of granite, which is to be seen on the western slope reaching nearly half way up, but the higher parts, it must be especially remarked, are composed of a hard hornblendic rock more nearly resembling basalt than granite. Numerous auriferous quartz reefs traverse this hornblendic part of the mount, one reef cleaving the very summit, while another reef attains the extraordinary thickness of thirty feet. It is true, indeed, that close to those reefs which

are now chiefly worked, a small quantity of soft whitish rock occurs, containing leaders or thin veins of quartz; but I am not sure whether this is to be considered Silurian schist rock. Even if it prove that there are here the remains of Silurian strata, formerly existing in much larger mass but now carried away by pluvial action to supply the alluvial drift of the surrounding country, it is still true that in another part of the mount the close conjunction of quartz reefs, hornblendic rock and granite is clearly established.

Adelong Creek is in New South Wales, not far from the borders of Victoria, and is remarkable for the most splendid single quartz reef yet opened, I believe, in Australia. The reef traverses bold steep ranges of a granitoid rock for a distance of a mile, and probably much further, in a line very nearly straight and not diverging more than two or three degrees of azimuth from the true meridional direction, as I ascertained by means of a prismatic compass.

This great reef is one continuous wall of quartz, cropping out at the surface and inclining, as it descends, ten or fifteen degrees from the perpendicular towards the west. The thickness of the quartz varies from almost nothing to six feet or more, "making" or "dying out," as the miners say (*i.e.* becoming thicker or thinner), in a most capricious manner. But the most instructive circumstance to the geologist is that a variety of igneous rocks occur close alongside the wall of quartz, appearing to support it, or at least to form bands parallel to it. The accompanying specimens will best show the nature of these rocks; they comprise felspar in a state of purity, and a series of hornblendic rocks containing successively larger proportions of white quartz. Hornblende was evidently a predominant mineral in the neighbourhood; but so various were the rocks which the miners had encountered, and so difficult was it

to learn particulars from them, that to arrive at the true nature and relative positions would have required a long personal examination, and also more geological experience than I possess.

Several other reefs parallel to the main reef, but of much smaller magnitude, were near, and were accompanied in a similar manner by hornblendic rocks. The richest parts of the reef were thought to be those where the quartz contained large quantities of the sulphides of iron, arsenic and copper.

Only very small quantities of gold had ever been obtained from the bed of the creek near, although the reef was proving highly remunerative to some thousands of persons engaged in mining and crushing quartz, in order to separate the gold by amalgamation. The yield was perhaps five (5) ounces of gold per ton, as much as ten (10) ounces being sometimes obtained.

Braidwood. — A very distinct kind of gold field is that near Braidwood, in the southern part of New South Wales. The country is a remarkable plateau of granite, uniformly elevated two thousand feet above the sea level. At Jembaicumbene gold is found in the wide shallow bed, or in the banks of a creek, surrounded by granite hills of very moderate elevation, or by flat lands from which granite boulders everywhere crop out.

A fine gold dust, quite free from large particles or nuggets, is found at the creek, lying on the granite bottom rocks, amid a detritus of purely granitic origin, including a fine white sand composed of minute perfect topaz crystals, which remain mixed with the gold in spite of all washing. Black magnetic iron sand is also found here in abundance, and quartz occurs in large crystals.

In the bed of the creek, indeed, I noticed some pebbles of extraneous origin; but I have not the least doubt that the Braidwood gold is derived directly from the surround-

ing granite country, being originally disseminated through that Plutonic rock.

Araluen is a long romantic ravine, bounded by unbroken granite ranges. Gold is found in the river bed amid granite boulders and detritus, and is evidently derived from the same source as that of Jembaicumbene, which is only six miles from the head of the Araluen valley.

I must mention that in the northern part of New South Wales (now within the new colony of Queensland), which I have never visited, gold is found in conjunction with quite a different set of rocks, viz. serpentine and its congeners. Such was the singular isolated mass of gold which occasioned the wild rush of ten thousand diggers to the tropical Fitzroy river a year since, only to meet there with hardship and complete disappointment of their golden hopes.

At Bingara and other parts of the north, diggings of a somewhat similar character were discovered and described by the well known geologist, the Rev. W. B. Clarke.

II. Speculations as to the Source of Gold.

From the order in which I have described the gold fields, the reader will almost perceive the view which I take of the geology of gold. It is doubtless the granite which originally contains the gold in a very finely disseminated state, as it is almost proved to exist at Braidwood. Now quartz is one of the chief constituents of granite; feldspar and hornblende are the other chief constituents. It has been proved,* too, that quartz is the

* "As a general rule, quartz, in a compact or amorphous state, forms a vitreous mass, serving as the base in which felspar and mica have crystallized; for, although these minerals are much more fusible than silex, they have often imprinted their forms on the quartz. This fact, apparently so paradoxical, has given rise to much ingenious speculation. We should naturally have anticipated, that, during the cooling of the mass, the flinty

portion which longest remains plastic, and that it often becomes segregated or collected together into fissures passing through the granite.*

I believe that a similar action on a much larger scale is the cause of all quartz reefs in Australia. Great cracks or fissures are produced in the granite by some cause with which we are unacquainted, passing upwards, also through the overlying stratified rocks. The more liquid quartz filters out of the surrounding granitic mass, carrying with it the gold and the metallic sulphides, and, soon filling the vacant fissure, is forced upwards and penetrates as far as possible the overlying strata. We have thus an explanation why hornblende is found in immediate connection with those quartz reefs which lie amid granite; for as the

portion would be the first to consolidate; and that the different varieties of felspar, as well as garnets and tourmalines, being more easily liquified by heat, would be the last. Precisely the reverse has taken place in the passage of most granite aggregates from a fluid to a solid state, crystals of the most fusible minerals being found enveloped in hard, transparent, glassy quartz, which has often taken very faithful casts of each, so as to preserve even the microscopically minute striations on the surface of prisms of tourmaline. Various explanations of this phenomenon have been proposed by MM. de Beaumont, Fournet, and Durocher. They refer to M. Guadin's experiments on the fusion of quartz, which shew that silex, as it cools, has the property of remaining in a viscous state, whereas alumina never does. This "gelatinous flint" is supposed to retain a considerable degree of plasticity long after the granitic mixture has acquired a low temperature."—Lyell's *Manual of Geology*, fifth edition, p. 567.

* "Veins of pure quartz are often found in granite as in many stratified rocks; but they are not traceable, like veins of granite or trap, to large bodies of rock of similar composition. They appear to have been cracks, into which silicious matter was infiltered. Such segregation, as it is called, can sometimes be shown to have clearly taken place long subsequently to the original consolidation of the containing rock. Thus, for example, I observed in the gneiss of Tronstad Strand, near Drammen in Norway, the annexed section on the beach. It appears that the alternating strata of whitish granitiform gneiss and black hornblende-schist were first cut through by a greenstone dyke, about two and a half feet wide; then the crack, *a b*, passed through all these rocks, and was filled up with quartz. The opposite walls of the vein are in some parts incrusted with transparent crystals of quartz, the middle of the vein being filled up with common opaque white quartz." —*Ibid.* p. 576.

plastic quartz is forced out, of course the hornblende and feldspar are left behind to form the walls of the reef. Among Silurian rocks we never, I believe, find hornblende near the quartz, and only in the instance of the feldspathic porphyry at Maryborough, do we find any plutonic rock at all present.

The gold districts of Victoria, including the southern parts of New South Wales, I regard as a great mass of auriferous granite, upon which lie Silurian strata of a slight thickness. Granite protrudes bodily from the surface in very many places, of which the May-day Hills, Mount Alexander, Mount Tarrengower, Adelong and Braidwood are only a few of the best known. In other parts the liquid quartzose constituent of granite has alone been forced upwards, filling a system of fissures which have an almost invariable direction from north to south. I am in no way called on to explain the cause of these parallel fissures, for the same parallelism is well known to be a characteristic of all systems of mineral veins, or fissures filled by mineral ores, in Cornwall and elsewhere.

Some of the opinions above expressed were entertained many years ago by the Rev. W. B. Clarke, of Sydney, a geologist who had discovered gold in New South Wales many years previously to Mr. Hargreaves's *practical* discovery, and who is better acquainted with the geology of Australia than any other person.

III. On the future Supply of Gold.

The question, Will the supply of gold from Australia increase, remain constant, or diminish? is of evident and high importance. I will therefore conclude with a few remarks by way of answer.

The rich diggings of Ballaarat, Bendigo and Forest Creek were all discovered in the course of one or two years after attention was first drawn by Mr. Hargreaves

to the presence of gold in the Australian rocks. Since then most favourable localities have been well prospected, or tested for alluvial gold by the many thousands of experienced diggers with whom Victoria now abounds. I regard it as certain, therefore, that no alluvial deposits, equally rich and equally accessible, will ever be laid open in future. But it is by no means certain that great and rich leads of gold may not exist in many places still concealed by the great thickness of the overlying alluvium, or by a portion of the basaltic formation or sheet of lava. Such leads would be comparable to the new and deep leads beneath the new town of Ballaarat, and would be discovered only by a very lucky chance, or by a skilful and expensive search. From the mode in which schistose hills, bearing fine auriferous reefs, here and there only just appear through the basaltic formation, as at Clunes and Melbourne, we may consider it very possible that complete gold districts may lie hidden, perhaps for ever, at considerable depths beneath other parts of the basaltic plain. Not much, perhaps, is to be hoped from the future discovery of alluvial gold.

The chief diggings of Victoria have, during the past eight years, been so vigorously undermined and turned up by small parties of miners that scarcely a square yard of untouched auriferous ground is left—at least such ground as would have formerly been considered profitable to men without experience and means of undertaking large works. But these diggings are by no means exhausted. The miners, learning the advantages of co-operation and the use of machinery, find that masses of alluvium, only slightly auriferous, now afford a handsome and also a steady profit. Ground exhausted in the old style is being reworked in a much more complete manner, and even the *tailings,* or refuse from the cradles of previous diggers, is valuable to those provided with more improved washing apparatus.

As science, capital and co-operation are more and more introduced, auriferous land of less and less richness will successively become profitable, and of such land the quantity, it may be stated, is certainly very great.

It follows then that the supply of gold, even from the present alluvial diggings, would diminish but slowly, unless indeed other social circumstances, as may well happen, should interfere. The many thousands of Chinese diggers, with their quiet plodding and, so to say, mean industry, assist to maintain and steady the supply. Yet it must certainly be allowed that the deposits of alluvial gold will begin to fail gradually in a very moderate period of time.

The answer is otherwise, I believe, when we take into account the quartz reefs which are real *gold mines.* The number of reefs now known to be auriferous is great; of the rest some may be devoid of gold, but the majority probably contain a proportion of the valuable metal which, either now or in some years to come, as machinery improves, will repay extraction. As reefs are now worked by small parties of miners, each owning a small claim and sinking an independent shaft, even the richest reefs would cease to be profitable at a certain depth, from the great comparative expense of draining and working so small a mine. But if a whole reef were in possession of one large monied company, two or three shafts and a single establishment would suffice; powerful engines and pumps would be employed to drain the mine, and the quartz reef might be followed as deeply as copper veins or coal beds. There is no known reason, beyond a mere fancy in the minds of some geologists, that reefs should become impoverished in sinking; the fancy is now proved to be contrary to fact. Even a few large reefs then would, I believe, yield a considerable and constant supply of gold for many years to come. For how much more may we not hope when greater experience is attained in quartz mining, now

so new an employment; when improved machinery is brought into use for the rapid, complete and cheap extraction of the gold from the quartz matrix; when capital is attracted in great sums to the pursuit; and when the search for new auriferous reefs, becoming more keen, is rewarded, as I believe it will be, by abundant discoveries?

Our conclusions, expressed as shortly and truthfully as possible, are:

1. That no great and recurring discoveries of alluvial gold are to be expected; so that the yield of alluvial gold must notably yet gradually fall off.*

2. That the supply of gold from its quartz matrix is subject to entirely different laws; that we at present know of no limit to the amount procurable with the aid of capital; and that that amount, whatever it be, will probably remain constant for a long period of time; that, in short, the supply of gold from Australia will prove as inexhaustible as the supply of tin and copper from the Cornwall mines, or as the supply of almost any other metal from its most common source.

* Since this paper was written, much sensation has been produced by the discovery of the Kiandra gold field in the neighbourhood of the Snowy Mountains. Of course further discoveries may take place in regions previously unprospected and almost unexplored. Thus the northern parts of the Australian Cordillera may contain rich gold deposits. But the opinions expressed above will *ultimately* apply to all such new regions, as they already apply partially to the principal gold fields now worked for nearly ten years.

VII.—*On the Vestiges of Extinct Glaciers in the Highlands of Great Britain and Ireland.*

By Edward Hull, F.G.S. *of the Geological Survey of Great Britain.*

Read February 7th, 1860.

It is with considerable diffidence that I venture to read the following paper before the Society, as it is one in which there is little that is new, and certainly nothing of pressing or immediate interest. But having during the last summer made a visit to the southern slopes of the Westmoreland mountains, with the object of tracing in some detail the impressions which it was generally known the ancient glaciers had left behind, I would fain hope that the communication of the results might not be uninteresting.

Besides entering upon this hitherto almost untrodden ground, I shall venture to present a short outline of similar glacial traces in the other highland regions of the British Islands; and here I would remark that there is a wide field for observation still open, and that what Professor Ramsay has done for North Wales, we want carried out in the mountains of Kerry and over the greater extent of the Scottish Highlands. From my own observations in the Lake district, I feel confident that, in order to arrive at an adequate idea of the extent and nature of the former glacial geography of the British Isles, it will be necessary

to examine in detail the flanks and main valleys of the mountain-groups, and the rock-surfaces of the districts by which those mountain-groups are surrounded.

It will be necessary to record on accurate maps of sufficient scale the directions of the striæ, and the levels to which they rise on the flanks of the valleys, the sites of the moraines both lateral and terminal, and the upper limits of the Northern Drift.

When this has been accomplished for any of our mountain-groups, it will then be possible to construct a map, reproducing with approximate accuracy the glacial system of the district, and the relative position of land and sea during the period. I can answer for it, that to one endowed with a love of nature, together with those essential accessories, time and money, the inquiry would afford ample enjoyment, and the scientific value of the results would not be unimportant. It was in a detailed survey of this kind that I was engaged during the summer; and as it is far from being complete, I hope to be able to devote another season to the northern flanks of the great central range which separates Westmoreland from Cumberland.

As far back as the year 1821 M. Venetz first announced his opinion, founded on ample testimony, that the glaciers of the Alps formerly extended far beyond their present limits.* These views were subsequently confirmed by MM. Charpentier and Agassiz, and are now universally received. But it was not until the year 1842 that Dr. Buckland† published his reasons for believing that the mountains of Caernarvonshire gave birth to glaciers which descended along seven main valleys; and that to these agents are to be attributed the polished, fluted and striated rock-surfaces which may be traced at intervals along the

* *Bibliothèque universelle de Genève*, tom. xxi.

† *Proceedings of the Geological Society of London* vol. iii.

pass of Llanberris and elsewhere. This opinion, at first received with incredulity, was subsequently confirmed by Mr. Darwin * and Professor Ramsay.†

The grounds upon which Dr. Buckland rested his conclusions were precisely those upon which M. Venetz inferred former extension of the Alpine glaciers. The effects of these streams of ice moving along their channels have now been repeatedly observed not only in central Europe, but in the Arctic regions, where they descend into the sea and give origin to icebergs. These effects consist in the polishing and moulding the bottoms and sides of the valleys into smooth oval bosses, or *roches moutonnées* — the production of striæ, flutings and scratches (which are generally parallel in a given locality); also, perched blocks and moraines. The combination of these phenomena in any region can only be attributed to the agency of glacial ice, as there is no other known power capable of producing them. When to these is added the dispersion of erratic blocks, or boulders of large size, over a district extending many miles from the parent masses to which they may be traced, we cannot hesitate to refer the transportation of these blocks to floating icebergs derived from glaciers in a manner similar to that which is in operation along the coast of Greenland,‡ or amongst the fiords of Tierra del Fuego.§

The only districts where, as far as I am aware, a detailed survey of the glacial striæ has been accomplished, are those of Snowdon by Professor A. C. Ramsay,‖ and the mountains of Scandinavia by M. J. C. Hörbye.¶ In the elaborate work of the latter, extending over the whole Nor-

* *Philosophical Magazine*, vol. xxi. p. 180.

† *Peaks, Passes and Glaciers*, 4th edition.

‡ *Quarterly Journal of the Geological Society of Loadon*, vol. ix.

§ Dr. Darwin's *Voyage of a Naturalist.*

‖ *Peaks, Passes and Glaciers*, 4th edition.

¶ *Observations sur les Phénomènes d'érosion en Norvège.* (Christiana.)

wegian Peninsula, but more especially devoted to the southern part of Norway, the directions of the glacial striations are indicated on beautifully executed maps, with great fidelity, and exhibit in the clearest manner the course of the ice, either as glaciers or bergs, at the period when almost the whole unsubmerged region was overspread by one broad winding sheet of snow and ice. From an inspection of these maps, it is evident that there was a general motion of the ice from the central axis of the chain at every point. Thus at the extreme north, the striæ point north; along the western coast, they point west; at the southern extremity, and on both sides of the Baltic, the normal direction is S.S.E. To this south-easterly drift of the ice it is owing that the plains of Germany, Poland and Russia, as far south as lat. 50°, are strewn with blocks of Scandinavian granite.

Reverting to the British Islands, I shall endeavour to present a short sketch of the glacial vestiges which are to be found amongst the mountains of Killarney in Ireland, of Caernarvon in North Wales, of the Lake district in England, and the Scottish Highlands. If the account of some of these districts is very meagre, it is because there are but few detailed observations for our guidance.

Mountains of Kerry.—Professor Agassiz, in giving a general sketch of the ancient glacial centres of the British Islands, includes amongst them the mountainous district at the southern extremity of Ireland, at the entrance to which are situated the far-famed Lakes of Killarney.*

On approaching this region from the east, it is impossible not to be struck with the vast accumulation of detritus, with large boulders derived from the rocks of which the mountains are composed. This deposit of the age of the northern drift is spread over the low-lying

* *Proceedings of the Geological Society*, vol. iii.

district of Carboniferous Limestone which extends to the lower lake. On the western and southern sides of this lake the mountains rise abruptly and attain at Carn Tual an elevation of 3404 feet, and here the glacial phenomena are as strongly pronounced as in any part of Wales and Scotland.

The Black Valley, one of the most wild and striking, which stretches from the head of the lower lake to the base of Macgillicuddy's Reeks, exhibits these appearances in their most marked form. The surfaces of the rocks are here worn into smooth oval bosses, lying with their major axes in the direction of the valley, and extending several hundred feet up the sides. These polished *roches moutonnées*, however, assume a singular appearance when traced into the upper lake. They rise above the surface in the form of small oval islands, lying parallel to each other, and, though frequently clothed with luxuriant vegetation, are generally smooth and bare. It is almost impossible to give an idea of these ice-moulded bosses, protruding their naked backs above the calm waters of the lake, bearing some resemblance to a number of up-turned hulls of ships, or to a shoal of whales swimming half out of the water.

Nearly all the main valleys present similar appearances. The rocks, wherever freshly exposed, are grooved and striated, as I had several opportunities of observing in the course of a short tour. The picturesque valley of Glengariff is specially remarkable for the freshness of the ice-groovings and scratches. Sir H. T. De la Beche* draws special attention to them here, and observes that the scoring and rounding of the bottom and sides of the valley, together with the striations, are unsurpassed by any similar examples in Ireland. These striæ point W.S.W., stretching along the valley till it is submerged in the sea at Bantry Bay.

* *Geological Observer*, p. 312.

Professor Agassiz considers that there have been other minor centres of glacial agency in Ireland, as amongst the Mourne mountains in Down and the mountains of Wicklow. Mr. Jukes, in the case of these latter, gives an instance of a boulder of granite twenty-seven feet long, which had been transported seven miles, partly across a wide valley.* Such a boulder, however, might have become imbedded in a stranded iceberg from some more northerly district, and have been subsequently carried to its present site. At the same time, Agassiz distinctly states the existence of lateral moraines in the mountainous districts south of Dublin and near Enniskillen,† and considers the Wicklow range as a centre of dispersion for erratic blocks.

North Wales.—The years 1841–42 appear to have been remarkably prolific in researches into the glacial phenomena of our islands, for we find Professor Agassiz, Dr. Buckland, and Sir C. Lyell announcing consecutively their convictions of the former existence of a state of things in these islands, which have their analogues only in Greenland, South Georgia, or Tierra del Fuego, at the present day. M. Agassiz pointed to the Caernarvonshire mountains as one of the centres of dispersion of glacial and erratic detritus; and Dr. Buckland speedily followed with details tending to prove that the seven valleys of Snowdonia were once occupied by as many glaciers, discharging loads of boulders and gravel over the lower grounds or into the sea, and covering the bottoms and sides of those valleys with flutings and furrows. He also shows that on the northern flanks of this district, boulders and marine drift coming from Anglesea, Cumberland, or

* *Manual of Geology*, p. 551.

† *Proceedings of the Geological Society of London*, vol. iii. p. 330. In the original, the district is said to be situated south-east of Dublin, but is evidently intended for south-west of that city.

Ireland, and containing, as shown by Mr. Trimmer,* marine shells, have been deposited at an elevation of 1392 feet on Moel Tryfane.

The observations of Dr. Buckland were followed by those of Mr. Darwin,† and more recently by those of Professor A. C. Ramsay.‡ This author has shown that many of the tarns, such as Llyn Llydaw and Llyn Idwal, have been produced partly through the damming up of the waters by moraines, as Agassiz had previously shown to be the case in the Alps, and Lyell in Forfarshire. The same author, in order to account for the fact that several of the mountain tarns, as those near the summits of Cader Idris, Moel Wynne and Snowdon, are in the form of basins hollowed in solid rock, has suggested an explanation which may be called "the scooping theory." These tarns are generally surrounded through half their circumference by precipitous walls of rock; and Professor Ramsay supposes, that solid masses of ice, descending from these heights, charged with imbedded fragments of rock, have actually scooped these hollows, which are so numerous in all mountain districts.

But there is one interesting fact brought out by Mr. Ramsay, and which, according to my own observation, is repeated amongst the valleys of the Lake district. Taking the moraine of Llyn Idwal as one of several examples, he shows that it is situated at about 1000 feet below the elevation attained by the Northern Drift. Now if this moraine had been formed previous to the deposition of this marine deposit (which attains an elevation of 2300 feet), it would most certainly have been entirely obliterated. It is therefore evident that moraines of this kind belong to a period

* Mr. Trimmer, on the Diluvial Deposits of Caernarvonshire, *Proceedings of the Geological Society*, vol. i. p. 331.

† *Philosophical Magazine*, vol. xxi. p. 180.

‡ *Peaks, Passes and Glaciers*, 4th edition; and *Quarterly Journal of the Geological Society*, vol. viii.

subsequent to the Northern Drift. Bearing this in mind, and recollecting the clear evidence which the *roches moutonnées*, frequently enclosed by marine drift, afford of having been formed by glaciers *before* the deposition of the same formation, we have here a sequence of three distinct, though connected, periods: the first, in which the glaciers descended down the main valleys; the second, when the land of Wales had sunk at least 2300 feet, during which the Till or Drift was spread over the flanks of the mountains; and the third, when the land had been elevated, and glaciers again descended from the heights ploughing out the Drift, and forming moraines for embankments to lakes and tarns.

The striations of the rock surfaces of Anglesea appear to be altogether disconnected with the glacier system of Caernarvonshire. The striæ and grooves generally range W. 30° S.,* and are probably the result of icebergs stranding and scoring the bottom as they floated from the mountains of Westmoreland.

THE LAKE DISTRICT. — The existence of former glaciers amongst the mountains of Westmoreland and Cumberland having been announced by Agassiz† and Buckland,‡ these great observers have left but very slight details of the phenomena upon which their conclusions were established. The truth is, that the evidence on this subject for all the highland districts of Britain is of so analogous a nature, and so incapable of being misinterpreted, that a few special cases were enough for their general purpose, that of establishing an interesting and novel theory.

Both these authors, however, notice in several localities on the southern and eastern sides of the district, examples of scored and grooved surfaces, and the mammilar bosses

* Ramsay, *Quarterly Journal*, vol. viii. p. 374.
† *Proceedings of the Geological Society*, vol. iii. pt. ii. p. 328.
‡ *Ibid.* pp. 345 *et seq.*

which occur at Penrith and Windermere. It appears to me, however, that Dr. Buckland has extended the glacial theory frequently beyond its true limits, and has mistaken, in the valley of the Eden, Walney Island, and elsewhere, remarkable forms of drift gravel and boulders for glacial moraines; and I must altogether dissent from the astounding supposition that a glacier stretched from the skirts of Shap Fell across the valley of the Eden,* by means of which the granite blocks were distributed over the high table-land of Stainmoor Forest and the valley of the Tees.

After a personal examination of a large portion of the Lake district, last summer, the details of which I have elsewhere recorded,† I shall endeavour to present a short outline of the glacial phenomena of this district. The watershed of the country crosses from Bow Fell on the west to Shap Fell on the east; and from this, branch off to the north and to the south a number of deep gorges which unite into larger valleys at some distance from the central ridge. The bottoms and flanks of nearly all these valleys present the usual striations, ranging parallel to their directions; and the rocks are frequently worn into *roches moutonnées* up to certain heights, well-defined along the sides. There are many interesting examples of perched blocks, such as at Stickle Tarn, where a boulder rests on a rounded boss rising slightly above the surface of the lake. In the lower and larger portions of the valleys we seldom find examples of true moraines, these being nearly confined to the higher portions near the central heights, as in the case of the two Langdales, Stockdale, Easedale, and the Stake Pass at the head of Borrowdale. Generally, the

* *Proceedings of the Geological Society*, vol. iii. pt. ii. p. 348. Mr. Bryce also notices the striated and polished surfaces of the rocks near Kendal, but refers them to the action of "waves and currents charged with detritus." — *Rep. British Association*, vol. xix.

† *Edinburgh New Philosophical Journal*, vol. xi. p. 31.

moraines assume the form of a collection of large boulder-strewn *cumuli*, or mounds, unlike anything I have ever observed beyond the limits of a glacier district.* There is, however, one remarkable exception to which I shall presently refer.

The rocks of a large district surrounding the interior mountains are remarkably ice-moulded, polished and striated, as far as the head of Morecambe Bay to the south, and the vale of the Eden to the north.

The Drift, a marine boulder-clay, rises to the height of 1200 feet on the southern slopes of the hills. Of its elevation on the northern flanks I cannot speak from personal observation. The clayey gravels, frequently bright red from the decomposition of the felspathic rocks, which occur at higher elevations, appear to consist of moraine matter.

Many of the small lonely mountain lakes, or tarns, have been formed partly by moraines thrown across their outlets. Amongst the most interesting examples of this kind are Easedale Tarn, Stickle Tarn and Blea Tarn. The formation of some others, such as the tarn that lies between Helvellyn and Fairfield, seems to be referable to "the scooping theory previously explained."

I have already stated that several well-marked moraines may be observed at elevations considerably below the upper limit of the Drift. All those occupying this position must, in consequence, be of more recent date than this marine deposit. Of examples of this class, the most remarkable which has yet come under my notice is the large terminal moraine at the lower extremity of Grisedale. This gorge, one of the most desolate and savage in Cum-

* See plates 1, 2, *Edinburgh New Philosophical Journal*, vol. xi. p. 31. This form of moraine appears to recur frequently in Scotland, according to the accounts of Sir C. Lyell and Dr. Buckland in the *Proceedings of the Geological Society*, vol. iii. p. 335, and pp. 340 *et seq.*

berland, descends from the heart of Helvellyn towards the head of Ulleswater. The rocks of porphyry which form the bottom and flanks of the valley, up to an elevation of about 500 feet on either side, are remarkably ice-moulded, affording numerous examples of perched blocks and lateral moraines. Striations are not, however, of frequent occurrence, owing to the nature of the rocks. On descending towards the mouth of the valley, the terminal moraine arrests the attention, and appears like a congeries of large rounded hummocks, strewn with boulders, rising up the sides of the valley to about 150 feet above the bed of the river. After the melting of the glacier, this moraine, in all probability, produced a lake. But the torrent has hewn a channel and levelled the ground over a breadth of about 100 yards. The position of this moraine is not more than 600 feet above the sea level, or 220 feet above Ulleswater; and it enables us to measure with exactness the dimensions of the glacier which formed it. Taken from the tarn at the head of the valley, this glacier was 3 miles in length, about 500 feet in depth at its centre, and from 200 to 400 yards in width. On the eastern side it was bounded by a continuous and nearly vertical escarpment of bedded trap; but the western side was very irregular and indented.

The phenomena of this region appear to show: first, a period when glaciers protruded far down the main valleys; secondly, an interval when the land was submerged about 1200 feet or more, during which the boulder clay was spread over the flanks of the hills and valleys; thirdly, a period when the land had been again elevated, and glaciers extended some distance down the minor valleys and ploughed out the Drift. It was a glacier of this third period which has left the terminal moraine of Grisedale.

Scottish Highlands. — The glacial vestiges of the Highlands of Scotland are on a scale more grand than

those of the Lake district or Wales, in proportion to the greater extent and loftiness of the mountains and their higher latitude. Ben Nevis in lat. 56° 50′ attaining an elevation of 4368 feet falls only a little short of the snow line, and is said to have patches of snow all the year round in the fissures near the summit.

The observations which have been recorded regarding the direction of the striæ, although they are lamentably few, go to prove that the Highlands formed a *centre of dispersion*, from which the ice-streams and bergs radiated in every direction from the central range.

The southern slopes of the Grampian Hills in Angus and Forfar have received a detailed examination at the hands of Sir C. Lyell, who has recorded his observations in the transactions of the Geological Society.* The striations follow the lines of the main valleys S.S.E., and several fine examples of lateral and terminal moraines are mentioned. Of these the great transverse barrier of Glenairn seems to be the most remarkable. The valley of the south Esk here contracts from a mile to half a mile in breadth, and is flanked by steep mountains. Seen from below, this barrier resembles an artificial dam 200 feet high, with numerous hillocks on the summit. Its breadth from north to south is half a mile. Sir C. Lyell considers this to be the terminal moraine of the *receding* glacier, and considers it probable that it once banked up the river so as to form a lake, which has since been drained by the Esk having cut a channel for itself 30 feet deep on the eastern side.

The Sidlaw Hills claim particular attention on account of the examples of transported boulders which they afford. Separated by the great valley of Strathmore from the Grampian range they reach an elevation of 1500 feet.

* *Proceedings of the Geological Society of London*, vol. iii. pt. ii. p. 357.

They are formed of Old Red Sandstone, and on their flanks at elevations of 700 and 800 feet are strewn blocks of gneiss and mica slate, which have been floated across the intervening space over a distance of fifteen miles. One of these blocks on Pitscanby Hill is thirteen feet long and seven across. This is an example on a much smaller scale of the erratic phenomena of the Alps, where enormous blocks have been transported across the great valley of Switzerland from the Mont Blanc range, and stranded along the flanks of the Jura hills. The *Pierre à bot,* one of these boulders, is one of the noblest monuments in the world of the transporting power of ice.

The Highlands of Perthshire have been examined along their southern watershed by Buckland and Agassiz,* who detail numerous examples of glacial traces in the shape of moraines, *roches moutonnées,* striæ and perched blocks. It is to be regretted that the former observer has scarcely recorded a single observation as to the directions of the striæ. M. Agassiz, however, mentions that along the valley of the Forth they range from N.W. to S.E., or in other words, radiate from the great line of the Grampians seaward.

Towards the southern extremity of the Highlands the same law obtains, and along the valleys of Loch Lomond and Loch Long the striations point south.† With regard to the former lake I have been informed by Professor Ramsay, that not only its sides but the rock-surfaces of its beautiful islands are completely covered with grooves and furrows.

In the western Highlands, Agassiz has observed that the direction of the striæ along the valleys of Loch Fine and Loch Long are generally S.E., and at Loch Etive and Loch Leven from east to west.

* *Proceedings of the Geological Society,* vol. iii. pt. ii.
† *Ibid.* p. 328.

The wild district of Inverness-shire and Ross-shire remains yet to be described as far as its glacial history is concerned. From what we know of the adjoining regions, however, we may surmise that its long channel-shaped valleys and arms of the sea, stretching from the coasts far into the mountains, must have presented a series of physical conditions very similar to that of Norway, where the glaciers appear to have descended into the sea during the glacial period.* The phenomena of Sutherlandshire appear to have forced such an analogy on the mind of Sir R. Murchison, when lately exploring this region. He says: "The spectator who, ascending the summit of Ben Stack, looks westward, observes between him and the sea of Scourie Bay a countless quantity of small loughs and tarns interspersed among the hollows of this brown-clad barren waste. On descending to examine the lower tract, he finds the surface frequently rounded off and polished like the *roches moutonnées* of the Alps; and observing numerous striæ usually divergent from the central mountains, and following the lines occupied by the principal lakes or maritime fiords, he can have no doubt that, in the glacial period the north-west of Scotland must have been very much in the present state of Greenland, as described by Rink: *i.e.* the central mountains occupied by snow and ice, from which vast glaciers are protruded to the lateral fiords or bays."†

Glacial vestiges are no less marked over the rugged and inhospitable island of Skye. Professor J. Forbes has carried his observations across from the Alps and Scandinavia to this remote region of Britain, and, in referring especially to the valley of Cornisk, states that "the grooves and striæ are as well marked, as continuous, and as strictly

* See Professor J. Forbes' *Travels in Norway*, and *Phénomènes d'érosion en Norvège*, par M. Hörbye.

† *Quarterly Journal of the Geological Society*, vol. xv. p. 361.

parallel to what I have elsewhere shown to be the necessary course of a tenaceous mass of ice urged by gravity down a valley, as anywhere in the Alps."*

Before taking leave of this district, I may observe that Professor Agassiz is of opinion that the parallel roads of Glenroy, near the foot of Ben Nevis, are attributable to a lateral glacier having been projected across the valley, near Bridge Roy, and another across the valley of Glen Speane.

By this means glacier lakes were formed, along whose margin the stratified terraces of gravel were produced which are now seen to line the flanks of the valley at a perfectly horizontal level through several leagues. The subsequent melting of the glaciers has entirely obliterated any traces of the agent by means of which the waters were pent up.† Mr. Darwin, however, takes a different view of the subject, considering that the parallel roads are marine terraces, formed during the submergence of the land to a depth of 1250 feet, their present elevation.‡

Having thus made a rapid circuit of the Scottish Highlands, I must now hasten to a close. I think, however, it is only due to so great authorities as Professor Agassiz and Dr. Buckland, that I should state that they appear to have considered "not only that glaciers once existed in the British Islands, but that large sheets of ice (*nappes*) covered all the surface of the districts surrounding the Highland groups." This opinion is founded on the wide extent to which unstratified gravels, perched blocks and polished *surfaces in situ* are distributed over the districts adjacent to the centres of distribution. I believe, however, that it is now generally allowed that floating ice, or rather *swimming* ice, has played a more important part in produc-

* *Edinburgh New Philosophical Journal*, vol. xi. p. 76.
† *Proceedings of the Geological Society*, vol. iii. p. 332.
‡ Lyell's *Elements of Geology*, 5th edition, p. 88.

ing these phenomena than was supposed by the founders of the glacial theory. It is indeed an almost unsolved problem, how we are, in all cases, to distinguish the effects of icebergs charged with stones scraping along the sides and bottoms of the channels through which they float, from the effects of subærial glaciers. If of large size, and impelled by prevalent winds or currents in one general direction, they would produce polished, grooved and rounded surfaces on the rocks with which they would come in contact, and leave behind blocks and *débris* strewn so as to resemble the matter of moraines. At the same time, there are several classes of objects which could *only* have been produced by subærial glaciers, and others which bear the unmistakeable impress of aqueous deposition.

I shall now only repeat, that the great object to be accomplished is the production of maps showing the direction of the striæ, the position of the moraines, and the limits of the drift, amongst the highlands of Britain.

VIII. — *A Brief Memoir of the late John Kennedy, Esq.* *By* William Fairbairn, Esq.

Read April 3rd, 1860.

The subject of the present memoir was born at Knockmalling, in the Stewartry of Kirkcudbright, N.B., on the 4th of July 1769. He was the third of five sons, and his father (who died at middle age) was a laird, living upon the paternal property which had descended through a long line of Kennedys for upwards of three hundred years. Of the education of his sons little can be said, as in such remote mountain districts no regular school was within reach, and they only received occasional instruction from some young graduate of the kirk, who took up his residence amongst the farmers in the winter months. In this way they were taught to read and write, though unfortunately what was learnt in the winter was in many cases lost in the summer, when all able to work were employed in the field, or in watching the sheep and cattle on the bare mountain pastures. Mr. Kennedy was, however, fortunate in receiving the instructions of Mr. Alexander Robb, who first directed his attention to the elementary principles of mechanics and mechanical movements.

On the death of his father, the whole management of the farm and of the family devolved upon the mother, who appears to have been a woman of strong good sense, who

brought up her children respectably, and laid the foundation of those qualities which distinguished them through life.

Mr. Kennedy was occasionally subject (like his father) to low spirits. He seems in early life to have been an observant, thoughtful child, and he always retained a most vivid recollection of the home life of his native district. He frequently, in later years, when speaking of the improvements which have been effected in the management of land and stock, used to tell how as a boy he felt for the poor cattle in the spring, which, from want of food, had become so weak that when once down they could not get up again, but had to be helped to rise from sheer weakness and want of support.

At the age of fourteen he and his brothers, excepting the eldest, who took to the farm, began to look about them for some employment by which they could earn a living; as it was then the custom in the great majority of Scottish families for the younger branches to emigrate, and make their way as best they could in a foreign land. At that time the great openings for employment were England, the West Indies, and America. The late Adam and George Murray, the founders of the well-known firm of that name in this city, who came from the same locality, and had settled as apprentices with Mr. Cannan, the machine maker at Chowbent, excited a strong desire in Mr. Kennedy to see new places and new things. He became ambitious to do something for himself, and to look beyond the still glens and blue mountains by which he was surrounded. With these feelings Mr. Kennedy was engaged as an apprentice to Messrs. Cannan and Smith, at Chowbent.

Early in February 1784, he started for England, with his mother's blessing. She told him "always to conform to the Presbyterian confession of faith, but (she said) to

remember that the Roman Catholic religion* and all others were alike, if you were sincere and acted up to their precepts, and they were all equally efficacious to salvation." With this sage advice and a new suit of home-spun, he left his home mounted on a pony, full of the events of his future prospects in life. After two days travelling he reached Carlisle, where he was met by his master's partner, Mr. Smith, who was employed in starting a carding-engine and one of Hargreaves' jennies, which were placed in different rooms, as there were no cotton mills at Carlisle at that time. From Carlisle he proceeded, in company with Mr. Smith, through Penrith, Kendal, and Preston, and on the sixth day reached his destination. At Preston he had an opportunity for the first time of hearing a lecture of Mr. Banks on Natural Philosophy, with which he seems to have been much interested, and which laid the foundation of his future tastes and desires in these pursuits. Immediately on his arrival at Chowbent he commenced work as a machine maker, and in a short time became an expert workman. The machinery made at that time was limited to carding machines, Hargreaves' jennies and Arkwright's water frames, including drawing and roving, then very imperfectly constructed.

At the close of his apprenticeship, which lasted seven years, he came to Manchester on the 18th of February 1791. Here he joined in partnership with Benjamin and William Sandford — who were fustian warehousemen — and Mr. James McConnel, under the firm of "Sandford, McConnel and Kennedy," and commenced business with them as machine makers and mule spinners. This was shortly after Crompton's invention of the mule, and the firm was for many years almost the sole makers of

* It is supposed that Mrs. Kennedy, a descendant of the old chevaliers and Catholic families, had some predilection for that form of worship.

Crompton's machine.* It will not be necessary here to give an account of the different machines which constituted the mechanism of a cotton factory in those days. I may, however, here observe that Mr. Kennedy rendered great service to the new system of mule spinning by the introduction of a new motion called the double speed, and which gave to the thread any amount of twist that might be required for the number of counts, or the quality of the yarn that had to be spun. This improvement of Mr. Kennedy gave greatly increased facilities to the spinning of fine yarns, and very soon enabled those engaged in that peculiar manufacture to raise their numbers from fifties and forties up to one hundred and fifties and two hundreds; and since that time numbers as high as fifteen hundred and two thousand have been spun. Still greater improvements have been made since that time, but less in the mule than in other machinery, excepting only the introduction of the self-acting mule; the increased fineness in the quality of the yarn of the present day being more attributable to carding, combing, and preparatory machines than to any of the improvements since made in the construction of the mule on which it is spun.

To show in what way Mr. Kennedy effected his improvements of Crompton's mule, he gives the following

* Crompton completed his mule in 1780, and after contending with many difficulties and annoyances he at last gave it to his competitors for a consideration that they never paid, but left the poor inventor a prey to poverty and the ingratitude of those who had benefited by his discoveries. This is another instance of the robbery — it cannot be designated by a milder term—practised upon the benefactors of the human race by the self-interest alloyed with ingratitude of the possessors of princely fortunes derived not from their own talents but from the higher intellects of those they have utterly forgotten and neglected. Such were the fortunes of Crompton and Cort, whose inventions have, along with those of James Watt, revolutionised the manufactures of iron and cotton, and given to their native country a predominance never yet paralleled in the history of nations. — See an excellent work by Gilbert J. French, Esq., entitled *The Life and Times of Samuel Crompton.*

account in his "Memoir of Crompton:" — "It was not until 1793 that any attempts were made in spinning fine yarns, say from one hundred hanks upwards, by power, when I observed the process very carefully. The rollers, according to the fineness of the thread, would only admit of a certain velocity per minute; for instance, with two hundreds, the rollers could only go ninety at the rate of twenty-five or twenty-six revolutions per minute, and the spindle about twelve hundred. But when the rollers ceased to move, then the spindle was accelerated by the spinner to nearly double its former speed. In what manner the acceleration of the speed of the spindle might be effected by machinery, without the aid of the spinner, had occurred to me, by observing in Mr. Watt's steam engine that one revolution of the beam (if I may use the expression) acting upon the fly-wheel by means of the sun and planet wheels, produced a double velocity. The difficulty, however, of making the necessary apparatus at that time induced me to use the more complicated method of four wheels of unequal sizes for producing the same effect. The description is as follows: — Two of the wheels were less and two larger; upon the rim axis one of the small and one of the large; and the two others were fixed in a frame which carried the axis upon which they were placed, and which had a shank or axis growing to it. This was placed in a vertical position, so that when the carriage was put up an arm projecting from this vertical shank was connected by a wire with a catch, which kept the lying shaft that turned the rollers in gear. In the elongating process the smaller wheel was in contact with the larger wheel upon the rim, but when by the disengagement of the catch the rollers became still or stationary, at that moment the larger wheel, by means of a weight, came in contact with the lesser wheel upon the rim or axis, to which it communicated a double velocity.

The shaft, with its large and small wheels working alternately had a pulley with a catch upon it, and was driven by the mill-work; and was forced into a corresponding catch upon the small shaft when the mule was to be set in motion by the steam power. The power in this instance was Savery's, which was used to raise water upon a water-wheel. There was a worm upon the rim axis, with a wheel upon it, the number of whose teeth determined the revolutions of the rim. The second drawing, which had generally been performed by hand, had also to be performed by the machine itself. This had been done in a few instances before power had been applied. From the simplest of these methods I took the hint, drawing a shaft from the rim by a strap from a small pulley upon the rim axis, and a large one upon the small axis which had the small pinion upon it; so that when the drawing-out wheel and band were disengaged from the front roller, they fell back into the small pinion whose axis was revolving at a very slow speed, and consequently gave a much slower speed to the second stretch or draw, as it is called, the speed of which was more or less, according to the numbers to be spun.

"Messrs. A. and G. Murray at that time, like myself and partners, were machine makers, and to a small extent we were both engaged in fine spinning by hand. They fitted up, upon the principle above described, a few pairs of hand mules, which they had previously made for one of their customers in Derbyshire, who had water power. Mr. Drinkwater, of Manchester, was the most extensive fine spinner at the time of which I speak. He was one of the early water spinners, and in possession of the most perfect system of roving making. His large mill in Piccadilly was filled with mules of one hundred and forty-four spindles each, all of which were worked by mens' hands. Mr. Owen, the philanthropist, was then his manager, and

they came to see the new machine in 1793. They approved of it, and thought it practical. Mr. Humphrey, of Glasgow, a good mechanic and millwright, succeeded Mr. Owen as manager, who also approved of the scheme, and got instructions to apply this steam power to the fine work produced by the mules in Piccadilly mill; and to make its advantages available he coupled two of one hundred and forty-four together, so that he saved one-half of the steam gearing, and obtained a reduction in the price of spinning, the spinner having double the number of spindles to operate upon. Mr. Humphrey made an improvement in the four wheels already described, by keeping them always in gear with a loose clutch between the two wheels on the rim shaft, which was alternately fastening the small driving wheel, and then relieving it and fastening the larger one which accelerated the speed of the rim, and furnished with a loose and fast pulley as already described. This prevailed for some years, when I thought it might be simplified, which was done by adopting three pullies, namely, one on the small wheel, another on the large wheel, and a loose pulley. The driving strap, which was on the loose pulley when the mule was at rest, was removed to the pulley on the smaller wheel when the rollers were to work, and then to the pulley on the larger wheel which accelerated the rim and spindles, until the thread was completed, when the strap being removed to the loose pulley, the whole machine came to rest, and the thread was put up by the spinner in the ordinary way. I was at this time able to construct the sun and planet wheels for the acceleration of the speed of the spindle in the following way: — The sun and planet wheels had only two wheels and one pulley with a clutch that fastened the sun wheel when the accelerated motion was required. But though this and many other modifications were introduced, the four wheels prevailed. Some of these, for convenience,

I constructed by making them bevils, and placing their axis vertically to get motion from an upright shaft, which produced the same effect as the spur wheels."*

In addition to his improvements of the mule, Mr. Kennedy was also the pioneer in forwarding the interests of the cotton trade, by improvements of the other machines employed in that manufacture. He was one of the first to suggest and carry out improvements in the roving frame, and the differential motion for winding the roving upon the bobbin owes much of its success to his sagacity and skill. For many years Mr. Kennedy carried on a series of experiments connected with this motion, and although it has been greatly modified in form and construction since his time, it still bears the impress of his mind, and remains the same in principle as when he experimented upon it. This beautiful and ingenious machine is also indebted to the late Mr. Ewart for some useful modifications; but the perfecting of the differential motion is due to Mr. Henry Houldsworth, of this city, assisted by the late Sir Peter Fairbairn, of Leeds.

As a proof of Mr. Kennedy's sound judgment on questions relating to mechanical improvement, he was consulted on the subject as to whether the Liverpool and Manchester Railway should be worked by locomotive or stationary engines. He was also appointed Umpire in the competition trials at Rainhill in 1830, and to his honour be it stated that this country and all others are indebted to him for advancing the railway system, by his appreciation of the different qualities of the engines, and his correct and just decision in this case.

As a spinner Mr. Kennedy was most successful in all his undertakings, and realised a large fortune. He was a most accurate observer, was endowed with a retentive memory, a clear perception, a sound judgment; and every

* This was done for Mr. Kennedy by the writer of this memoir in 1823.

discovery in mechanical science received his cordial support. He was a friend and admirer of Watt, and there were few distinguished men in the scientific world with whom he was not acquainted, and on terms of friendly intercourse. Round his table were at all times to be found men who were noted for intellectual acquirements. With the distinguished men of Manchester and the surrounding districts he lived on terms of the closest intimacy; and although he did not attend regularly at the meetings of this society, in consequence of a dislike to the public expression of his opinions, he nevertheless took a deep interest in its proceedings; and during the whole of a useful life remained the friend of Dalton, Henry, and other men eminent for their discoveries and writings in science.

Mr. Kennedy was a man of sterling honesty in all his transactions. He began life at a time when the cotton trade was in its infancy, and he lived to see it attain its present colossal dimensions. As a man of business he was successful, but it is doubtful whether his tastes and talents would have fitted him for the present system of free trade, and whether he would not have been distanced by more energetic and active competitors. That this would have been the case is more than probable, as he was of a nervous temperament, subject to great depression of spirits, which might have paralysed his exertions, and prostrated him in a contest to which he was unequal. As it was he attained honour and success, and he lived at a time when business matters were easy, and when skill and practical science were much in demand.

Mr. Kennedy never pursued business for the sake of money, but for the love of improvements in his favourite mechanical pursuits. To these he devoted nearly the whole of his time, and there was scarcely any discovery in the arts that he did not make himself acquainted with.

He was fond of mechanical discussion, and never lost an opportunity of conversing with ingenious workmen, or those who were entitled to respect for their skill. Hence he visited all the workshops, and had no greater pleasure than when having a "crack" with an intelligent, well-informed workman. To the young he was always kind and communicative, but according to the custom of the times he expected that young people should never dispute the wisdom of their superiors, but thankfully receive the information afforded to them. This feeling was almost universal amongst the immediate successors of Watt, and many of them would admit of no mechanical improvement unless it originated in the school at Soho. This weakness was not always agreeable to rising merit, and was foreign to the great men for whom this partiality was shown, and it often had an injurious tendency, in so far as it damped the energies of the more modest and deserving aspirants. Mr. Kennedy nevertheless freely extended his patronage and friendship to those who were entitled to his confidence, and when once given it was never withdrawn. He had a high sense of honour in his friendships, and never allowed another to depreciate qualities for which he personally entertained feelings of respect.

In private society Mr. Kennedy had the manners and conversation of a gentleman, acquired, not from his education, but from his subsequent intercourse with the best society. He had great discrimination, and would never associate with any but those of superior attainments, and hence the attraction of his opinions and conversation. He was full of anecdote, old sayings, and sage remarks, and few could tell a story with more zest. The sayings of others and the anecdotes of early life he gave with a dry humour that never failed to produce a pleasing effect.

At the time of his death, which took place on the 30th October 1855, at the advanced age of eighty-six years,

he was the oldest member of the society. He was elected in April 1803, and during a long period of years was a regular attendant at the meetings. His writings were few in number, but he contributed to the society's memoirs four valuable papers, "On the Rise and Progress of the Cotton Trade," read November 2, 1815; "On the Poor Laws," read March 5, 1819, a paper much spoken of at the time, ably reviewed in the *Edinburgh*, and not without results in the improvements which subsequently took place in the amendment of those laws. The next, entitled "Observations on the Influence of Machinery on the Working Classes of the Community," was read February 10, 1826; and a brief "Memoir of Crompton," with a description of his mule, read February 20th 1830, was the last of his literary efforts. In these communications he displayed consummate judgment, and a thorough knowledge of the subjects on which he treated; yet it must be confessed that the views he entertained upon the subject of free trade and national intercourse have since that time been greatly modified, and a totally different system of commercial relations between this country and the rest of the world has been adopted.

IX.—*Suggestions for a New Form of Floating Lightship, and a mode of estimating the distances of Lighthouses.* *By* Alfred Fryer.

Read January 10th, 1860.

The objections to the present form of floating lightships are well expressed by an eminent authority in pharology as follows:

"Floating lights," writes Alan Stevenson, C.E., "are very expensive, and more or less uncertain, from their liability to drift from their moorings; they should therefore never be employed to indicate a turning point in a navigation in any situation where the conjunction of lights on the shore can be applied at any reasonable expense." Objectionable as this method of lighting is, it is in many cases the only one that can be adopted; and no fewer than twenty-six floating lights surround the English coast. As the injury to which they are liable arises from the violent assaults of wind and waves during storms, the problem appears to be, "What is the form of vessel that shall be most secure from the action of storms?" The following is offered as a solution for cases where there is sufficient depth of water: The proposed lightship somewhat resembles a hydrometer, the mass of which (the bulb) used as dwelling and store rooms, is removed from the action of wind and waves, and the stem presents the smallest possible surface for their violence. The structure,

which is represented in the accompanying drawing is of wrought iron; the tube or stem at its upper end is of a diameter sufficient only to admit the body of a man easily, and at its lower end the diameter is doubled to give increased strength. The lantern, with light-room beneath it, and gallery attached, terminates the superior end of the tube, while its inferior extremity is riveted on to the dwelling place of the attendants, which is the frustrum of an inverted cone with convex ends. To the base of the cone and in its plane a wide annular flange is securely stayed, and the vessel is weighted by means of pigs of iron stowed in the hold and a cast iron sphere suspended beneath, so that the dwelling apartment is sunk beneath the surface of the water, and beyond the reach of the action of the waves. The exact form and dimensions will depend in part on the depth of the water, the range of light required, and the exposure of the situation. The following are the chief particulars of such a lightship as is represented in the diagram:

With the exception of the brass-work and glass of the lanterns, the vessel is constructed entirely of wrought iron. The plates of the submerged part and those within reach of the waves are very strong, but thinner ones are used for the upper part. The upper lantern is five and a half feet in diameter, the light-room six feet in diameter and six and a half feet high. The whole structure is one hundred and twenty-seven feet in length, two-thirds of which are exposed, and one-third submerged. The tube, two feet and a half at the top, increases to five feet in diameter at the water-line, and is seven feet when connected with the dwelling. The tube, which is divided vertically into two shafts of unequal size, furnishes means for efficient ventilation. An iron ladder is secured in the larger or downcast shaft, while the smaller or upcast shaft encloses the smoke flue. The dwelling apartment consists of a cir-

cular room eighteen feet in diameter and eleven feet high, and below it is the store-room fourteen feet wide and seven feet in height.

The structure weighs fifty-five tons, and when sunk to the required depth displaces five thousand one hundred and thirty feet of water or one hundred and forty-eight tons. To counteract the buoyancy eighty-seven tons of ballast are required, thirty-one of which are suspended from the bottom as a sphere of cast iron seven feet in diameter, while fifty tons of pig iron cover the floor of the store-room; the remaining weight of six tons is in stores.

The upper light, eighty feet above the water, can be seen at twelve miles distance; and the lower light (the object of which will be explained afterwards) being thirty feet above the water, can be seen at about eight miles distance. The centre of gravity is thirty-three feet below the surface of the water, and the centre of buoyancy twenty-six feet; thus the vessel is stable, and exerts considerable power to regain the perpendicular if removed from that position. The large disc mentioned above is intended to steady the vessel and to reduce oscillation, as no movement of a vibratory character can take place without the flange displacing a proportionate quantity of water. The inertia of a mass of one hundred and fifty tons, and the time and power required for the disc to displace large volumes of water, cause the vessel to be slow to obey the proportionately slight impulse to rise and fall with the waves.

The surface on which the waves can beat is very small, and is in fact less than the aggregate surface of the legs of a screw-pile lighthouse. The wind also can exert but little force upon so narrow a tube, which appears scarcely thicker than a large ship's mast, and like it presents a taper form and convex surface. The structure is moored by three anchors, the cables from which are attached to

the edge of the disc, and at equal distances. The vessel here represented cannot float in less than seven fathoms water, though it is possible that with a slight modification of form a lightship could be adapted for certain situations that would rest on the bottom, or float, according to the height of the tide. Where the depth of water will allow of it, the structure would be improved by keeping the centres of gravity and buoyancy further apart than here proposed. This would be best accomplished by enlarging the capacity of the upper part of the dwelling, and placing the ballast still further from the surface of the water.

The discomfort that would arise from a residence five fathoms below the surface of the water is found when examined, to be at least as little objectionable as that attendant upon any floating dwelling away from land. The inhabitants are safer than in a lightship of the usual construction or in an ordinary vessel, there being slight danger of being capsized, drifted away, or wrecked. They are safe from the fury of the waves, and protected from the violence of the wind. They do not fear that their vessel may be consumed by fire, or injured by strokes of lightning. From the shape and strength of the lightship there is little chance of foundering; but, should it spring a leak, the spherical weight can be released from within, when the vessel will rise to the surface, but still remain vertical; the hatches can then be made up, a signal hoisted, and assistance waited for. The dwelling, even during storms, is free from motion, and secure from noise; it is, moreover, protected from the frosts of winter, and the intense heat of summer. Some daylight may be introduced down the tube by means of reflectors, but this is of little importance, as the services of the attendant on duty are required on the gallery or in the light-room. No better plan could be devised for securing ample ventilation. The danger to be apprehended from being run down

by vessels is less to this construction than to pile lighthouses or ordinary lightships, as less surface is presented for collision; but if actually struck, the convex surface and extra-strong plates would most likely cause the tube to glide aside uninjured.

Objection will not be taken to this plan on the ground of expense. Two thousand five hundred pounds would probably cover the cost, including ballast, but excluding lanterns and illuminating apparatus.

As the oscillations of this vessel will be slow and small, it is possible that the dioptric method of illumination could be used, especially if the apparatus were judiciously suspended, and furnished with a contrivance for retarding the oscillations. Experiment only, however, can determine this.

Accidents not unfrequently occur from the mariner mistaking his distance from a light. Estimating it to be five miles distant he may find, when upon the rock or shoal he wished to avoid, that a slight haze had partially obscured the light and that he was within a mile of it. *Double lights,* or lights exposed from different elevations in the same tower, are now used in some cases, but solely I believe with a view to prevent confusion, by giving the light a distinctive character. I would propose, in all cases where it is important that the distance from a light should be ascertained, that double lights should be employed. Sailors would learn to estimate their position approximately by the apparent separation of the lights, and the distance could be determined with accuracy by the use of the sextant. The lights here represented are fifty feet apart, and as the height of the lower is thirty feet above the surface of the water, they would be visible at eight miles distance, and the combination would then be clearly discerned as a double light.

If Mr. Herbert's proposal to moor a series of floating lights along the middle of the English channel should find acceptance, the plan here proposed would be specially applicable on account of the depth of water. In that case it is suggested that each vessel should become a telegraph station, and by means of a cable each would be in communication with the others, with the shore, and with London. It is already the duty of light-keepers to report wrecks, vessels in distress, and even vessels sighted. These reports could then be made instantly to head quarters, and the information would be received when it was of most value. Injury to the illuminating apparatus, to the vessel, serious illness of a light-keeper, or deficiency of stores, could be reported at once, and the requisite assistance could be early afforded. In addition to this each lightship would occupy the post of a sentinel, and the time may come when the information they could give might be of importance to the Government.

The paper was illustrated by models of the ordinary and proposed lightships; and the relative steadiness of the latter was shown when the water was agitated, or equal force applied to disturb the equilibrium of each.

X.—*On the Influence of Atmospheric Changes upon Disease.*

By ARTHUR RANSOME, M.B., *B.A. Cantab.* M.R.C.S., *and* GEORGE V. VERNON, F.R.A.S., M.B.M.S.

Read April 17th, 1860.

Two hundred years ago the remark was made to Sydenham,* and the statement holds true now, that "no physician hitherto has attentively considered the force and influence of the atmosphere upon human bodies; nor yet has he sufficiently ascertained the part it plays in prolonging human life."

From very early ages men have observed that certain diseases prevail most during certain seasons, and have ascribed to atmospheric changes an important influence upon health, but, until recently, no solid foundation of accurately observed facts had been laid.

Most of the old medical writers deal with the subject—some of them very carefully. Hippocrates devoted one of his works to "Airs, Waters and Places;" and his writings upon epidemics, and his aphorisms, abound with remarks on the influence of various states of the air upon the human frame. Since his time many others have very fully noticed the coincidence between these phenomena, as

* Letter to Dr. Sydenham, from Dr. Thos. Brady.—*Sydenham Society's Transactions*, vol. ii. p. 1.

Aretæus,* Sydenham,† Boerhaave,‡ Vitet, Ramazzini,§ Baglivi,|| and more modern authors. Still, owing perhaps to the imperceptible and apparently mysterious way in which atmospheric changes take place, and more to the necessity for well organized and simultaneous observations in both branches of the inquiry, little further progress towards a true science of medical meteorology has been made until lately. The first attempt upon a comprehensive plan to advance this subject was made in January 1844, by adding meteorological tables, furnished by the Astronomer Royal, to the weekly returns of the Registrar General, these returns being for London only. Somewhat later, monthly returns were obtained from stations in different parts of England, and appended to the quarterly returns of the Registrar General; and from the year 1849 these stations have gradually increased in number, and at the present time there are about sixty in England and Wales. Similar returns from about forty-five stations are now added to the monthly and quarterly returns of the Registrar General for Scotland.

Some years after these returns were commenced, it was thought that more useful information might be obtained by similar comparisons with respect to disease; and in 1853 an attempt was made by some members of the Provincial Medical Association to compare meteorological tables for different places, with the diseases prevalent in those districts, but unfortunately these records were not continued for more than two years and a quarter.

In 1857 the General Board of Health in London took up this question; and from the week ending April 11*th*

* *περι Αἵματος 'Αναγωγῆς.*

† *Observationum Medicarum.*

‡ *Causes of Disease.*

§ *De principium valetudine tuenda,* cap. iii.

|| *De æris influxibus investigandis ac perdiscendis, ad morbos dignoscendos et curandos.*

1857, to the week ending November *6th* 1858, they published a carefully compiled weekly return of new cases of disease in London, furnished by the voluntary efforts of upwards of two hundred gentlemen connected with the medical profession. The tables are accompanied by meteorological observations made at six stations in and out of London, and although not perfectly accurate, yet they are of great value; it is much to be regretted that they were carried on for so short a period.

Hitherto careful collation of the two classes of facts recorded in these tables seems to have been wanting; and in the present paper we have endeavoured to supply the deficiency, and to deduce from the comparison some general conclusions.

We must here state that our inquiry originated in some investigations which were made for the Manchester and Salford Sanitary Association by a committee consisting of Messrs. Curtis, Ransome and Vernon.

The method we have employed in making the necessary comparisons of the two series of observations has been as follows:

1*st*. We have projected the medical and meteorological returns upon separate charts, so as to form curves, which represent the prevalence of the disease or the state of the atmosphere at any particular time; and then, by comparing the two charts, and noticing any evident coincidences, we have been led to the conclusions specified in the paper, respecting the following diseases: Diarrhœa, dysentery, pneumonia, bronchitis and catarrh, pleurisy, continued fever, rheumatic fever, measles, whooping cough, and scarlatina.

Diarrhœa.

A high mean temperature (above 60°) would seem to have a powerful influence in predisposing to this disease;

when continuous, causing a rapid increase in the number of cases.

A temperature below 60° appears to be unfavourable to its progress.

The above action is generally most evidently shown when the temperature is above or below the average of the season.

In the spring of 1857, from April 11*th* to June 20*th*, there is a gradual, and at first scarcely perceptible, rise in the diarrhœa curve, the number of cases being comparatively very small.

The temperature in April and the early part of May is much below the average (8° on May 2*nd*), although, on the whole, gradually rising.

From June 20*th* to July 11*th* the rise of the disease curve to 2,000 cases is more rapid — the temperature is above 60°, and on June 27*th* 7° above the average.

From July 18*th* to August 15*th* there is a very great increase in the number of cases (even to 5,600), but from the latter date until September 12*th* the curve sinks at nearly the *same rate* to 2,200 cases.

The number of cases then continues to diminish, but at a rather slower rate, until October 10*th*, when it is 600.

The mean temperature during the *whole of this period*, from July 18*th* to October 10*th* is above 60°, and considerably above the average, sometimes as much as 7°. In the weeks ending July 18*th* and 25*th*, it is stationary at 68° (the highest point this year), but it then gradually falls to 63·5° on August 15*th*; and after a temporary rise in the week ending August 29*th* it continues to fall until October 10*th*, thus throughout bearing a close relation to the disease.

From the week ending October 10*th* (the temperature being 1·5° below the average) the number of cases still

remains low (still diminishing as the temperature falls until January *9th*), and it does not again rise until May *22nd*, 1858.

In the two weeks preceding May *22nd* 1858, the temperature is below the average as much as 6°, but it now begins rapidly to rise, and from May *29th* to June *26th*, 1858, it is considerably above the average (on June *5th*, nearly 10°).*

In accordance with the rise of the temperature curve the number of cases increases, and continues to increase steadily, as in the preceding year, until July *10th*, when it is 1,200 (on July *1st* 1857 it was 1,400).

But a remarkable difference between the two years must now be noticed, as it affords a striking illustration of proposition (*b*).

In 1857 the disease runs on after July *11th* to an amazing prevalence, but in the present year (1858) there seems to be a sudden arrest, the number of cases remains almost stationary for a fortnight, and then slightly diminishes until August *7th*. When we inquire into the causes of this difference, we find that whereas in 1857, from June *20th* to September *26th*, the mean temperature never sinks below 60°; in 1858, for the first two weeks in July, the mean temperature is below 60°; and on July *10th* nearly 6° below the average. It seems as if the germs of the disease were so far destroyed by the unusual cold, that even the moderate warmth that follows could not again rouse them into activity.

The mean temperature in 1858 does not remain above the average, as it did in the preceding year.

From August *7th* to August *28th* 1858, the diarrhœa curve rises and falls with the mean temperature, but on August *28th* the thermometer again sinks below 60°; and although it again rises in September to 63·5°, it is accompanied by no corresponding increase in the number of

cases,—the diarrhœa agent has again received a check from which it does not recover.

Dysentery

(1) Seems to be influenced by the variations in the mean temperature, but in less degree than diarrhœa, the effect not being generally traced in the lesser undulations of the curve.

(2) Increased atmospheric pressure seems to be unfavourable to the progress of the disease, high readings of the barometer being nearly always accompanied by diminished prevalence of dysentery.

The dysentery curve rises, on the whole, from the week ending April 11*th* to the week ending September 12*th.* Fostered by the unusual warmth of the season, the disease seems to gather such strength that for a fortnight after the mean temperature begins to decline, it rushes on to still greater prevalence, and reaches its highest point when the mean temperature has fallen from 68° to 60·5.°

The diminishing autumnal temperature, however, seems at length to produce an opposing influence, for the disease from this point gradually subsides, with occasional fluctuations, until the week ending January 16*th.* There is then a sudden temporary rise in the disease curve, the mean temperature being now above the average, but having been very variable in the preceding three weeks.

During February 1858, there is a rapid increase in the number of cases which is associated with a temperature very much *below* the average (as though great cold as well as great heat were favourable to the disease); but it must be noticed at the same time that the barometric reading during the month was very low.

The disease curve now falls until April 17*th,* and continues low until June 19*th* (nearly the same date as that

on which the disease took its first decided rise in the preceding year). The mean temperature has now been very high for a fortnight (from 8° to 10° above the average); and the number of cases rapidly increases until July 10*th*, when it may be noticed that the mean temperature falls suddenly to 56° (6° below the average), and the further progress of the disease is checked.

After a short rise on July 24*th* (the mean temperature having then again risen 5° above the average for the week) the dysentery curve now gradually subsides, with many fluctuations, until October 2*nd*; and it may be noticed that the most decided rise is in the week ending September 25*th*, following the unusually high temperature of the preceding week (64° or 6·5° above the average).*

Pneumonia

Seems to be very greatly influenced by the mean temperature, the disease curve rising as the temperature falls, and *vice versâ*.

The above statement receives its best illustration in the spring, summer, and autumn of the year 1857.

In the early part of the year, while the temperature remains low, the disease is still prevalent, but as the

* Hippocrates, *Aph.* 22, book iii., speaks of dysentery as an autumnal disease: "With regard to the seasons, if winter be of a dry and northerly character, and the spring rainy, and southerly, in summer there will necessarily be acute fevers, opthalmies, and dysenteries, especially in women, and in men of humid temperament." — *Aph.* 3, xi.

Sydenham mentions dysentery, amongst other diseases, "which commencing in August run on to winter."

In the report upon the status of disease, drawn from returns made at the time of the census of Ireland for the year 1851, Messrs. Donnelly and Wilde conclude that diarrhœa and dysentery prevail more in the summer and autumn than at any other season.

"They occur in the season of summer; next in autumn; less in spring; least of all in winter." — Aretæus, *On the Causes and Symptoms of Chronic Diseases*, book ii. ch. ix.

warm weather advances it gradually declines, and remains low throughout the unusually warm summer, being least prevalent when the mean temperature is highest in July and August. The number of cases begins to rise in the latter end of August, and reaches its maximum on November 28*th*, the mean temperature being then 42°.

During this period, the way in which the two curves of mean temperature and pneumonia supplement one another is very remarkable. From April 11*th* to November 28*th* (thirty-four weeks) there are only seven exceptions to this rule, and when we examine these we find that most of them may be accounted for without much difficulty.

The first of the exceptions occurs in the week ending May 9*th*, when the disease curve rises considerably, the temperature also rising, but it must be remarked that the temperature is still 7° below the average, and that in the preceding week it was 7·5° below the average.

In the week ending May 23*rd* another, but very slight, deviation from our rule may be noticed, — the pneumonia curve continues to descend, while there is a slight fall (half a degree) in the temperature.

In the week ending June 20*th* there is a temporary rise in the number of cases, together with the mean temperature, but this seems again to be accounted for by the occurrence of a temperature 2° below the average in the preceding week.

In the week ending August 29*th* there is a slight rise in the number of cases, which cannot be accounted for by any fall of the mean temperature (68° or 9° above the average). (The N.E. winds prevailed this week, following a long continuance of S.W. winds, and the degree of humidity rapidly fell.)

In the week ending September 26*th* the number of cases diminishes during a falling temperature, but in the preceding week the mean temperature was 5·5° above the

average. Lastly in the week ending November *7th* the onward course of the disease does not seem to be checked by the temporary but unusual heat.

In the spring of 1858 the very close accordance between the two curves is not observed. Although the mean temperature falls lower than it has yet done, and the number of cases of pneumonia is still very great, yet it never again reaches the height that it did in November.

It must, however, be observed that the highest point of the curve this season corresponds with the period of the greatest cold, the week ending March 13*th* having a temperature of 35° (6° below the average), the preceding week being still colder (32° or 8·5° below the average). The waves of the disease curve apparently lay behind those of the mean temperature. (The humidity is now diminishing, and N.E. winds very prevalent.)

The mean temperature now begins to rise, and the disease diminishes in prevalence on the whole until August 21*st*, many fluctuations intervening, until with the advancing cold of the autumn an increase again takes place.

In the lesser modulations of the two curves from April 10*th* to June 26*th* (eleven weeks) there is again a very close correspondence, there being only one exception in the week ending May 8*th*; the disease curve then falling, after a short rise, while the mean temperature continues to diminish.

From June 26*th* to July 24*th* there is apparently an important departure from our rule. In the weeks ending June 26*th*, July 3*rd*, while the mean temperature is falling, the number of cases of pneumonia continues to diminish. It seems probable, however, that this may be owing to the unusual heat of the preceding week (69° or 8·5° above the average), and the discrepancy in July seems to be due to the disease curve rolling up behind that of temperature, the rise in the pneumonia curve following the unusual fall

in the mean temperature of the preceding week (to 6° below the average).

In the week ending August 21*st* the departure from our rule is very slight; the disease curve continues to fall together with the temperature, apparently in consequence of the continued influence of the heat of the preceding week, which is 3·5° above the average.

In the week ending September 18*th* the disease curve rises, and in that ending October 2*nd* it falls in accordance with the curve of temperature, but in the latter instance the preceding high temperature seems to display its influence.

Out of the seventy-nine weeks which we have now examined, twenty-three (29 per cent) exhibit departures from exact accordance with our rule; but, as we have seen, most of these are still to be accounted for on the supposition that the mean temperature influences the progress of the disease; but it seems probable that other elements, such as N.E. winds, also exercise some effect.

Bronchitis and Catarrh.

The curve of these diseases, although drawn from ten times the number of cases, is almost identical with that of pneumonia, its highest and lowest points coinciding exactly with those of the pneumonia curve.

It will be unnecessary, therefore, to trace it throughout its course, since it is evidently affected by temperature in much the same way as pneumonia.

The correspondence of the mean temperature curve with that of bronchitis is even closer than with that of pneumonia, the exceptions being only 26½ per cent.

It may be observed that in the year 1857, when the disease curve marks a deviation from the rule of temperature, it may generally be ascribed to a change in the degree of humidity, the disease curve rising as the amount of moisture diminishes, and *vice versâ.*

The chief discordance between the pneumonia and the catarrh-bronchitis curves takes place in the latter end of September and in October, which may possibly be due to the greater influence of the moisture upon bronchitis and catarrh than upon pneumonia; the degree of humidity at this time rises rapidly.

In June and July 1858, the catarrh-bronchitis curve seems to answer more rapidly to the influence of the temperature than the pneumonia curve does.

Pleurisy.

This disease is too irregular in its course to yield any information in the present investigation, as the meteorological elements under consideration do not appear to have any apparent connexion with it.*

Continued Fever.

It is difficult to trace any connection between the progress of this disease and the meteorological elements under consideration, but on the whole high temperatures seem rather favourable to its production, and extreme cold is probably opposed thereto.

From April 11*th* 1857, the *fever curve*, frequently fluctuating, on the whole ascends until November 7*th*, when a sudden fall takes place, and it sinks rapidly until February 13*th*. In the first part of its course, from May 9*th* to August 29*th*, it accompanies the rise of the mean temperature, but after the latter begins to fall the fever curve goes on rising as steadily as before for two entire months, and is not affected by the advancing cold until the week ending November 14*th*, when the thermometer stands at 45°.

* Among the seasons of the year, winter more especially engenders the disease, next autumn, spring less frequently, but summer most rarely.—Aretæus, *Causes and Symptoms of Acute Diseases*, book i. chap. x.

As though the heat had called into activity some agent which resisted moderate fall of temperature, but which was destroyed by the cold of November, December, and January.*

During the whole time of the gradual increase of the disease the mean temperature is throughout above the average.

From February 1858 the fever curve does not rise much until the week ending June *5th*, when a sudden increase of 110 in the number of cases accompanies a rise of 12° in the mean temperature.

From April *11th* 1858 to October *23rd*, there is the same gradual advance in the disease curve as in the corresponding period of 1857, with the exception of the month of June 1858. The temperature this month was excessive, and to this in a great measure must be attributed the sudden rise in the number of cases. The month of July, which followed, had a temperature considerably below the average, and this checked the rapid advance of the disease for a time; but it will be seen that leaving the month of June out of the question (as being abnormal) the curve gradually ascends up to October *23rd*, when our observations end.

Of the lesser modulations of the fever curve 61 per cent take place in accordance with the variations of the mean temperature, the disease rising and falling with the temperature; and in many of those weeks which present deviations these seem to be due to the lagging of the disease curve behind that of temperature; as in the weeks April 18, 25, May 2, June 6, 13, August 15, 22, 29, and September 5 and 25, 1857; also March 27, May 1, June 26, September 4, 11, 1858.†

* Drs. Donnelly and Wilde remark that fever, although very prevalent in spring, seldom rises to its intensity until summer and autumn.

† It takes birth when spring passes into summer, and it rises towards

Rheumatic Fever.

The curve of this disease is not sufficiently extended to admit of accurate comparison with the meteorological curves, and therefore no decided conclusion can be drawn respecting it.

Our data, however, would bear out the observation of Sydenham: "This disease may come on at any time; it is commonest, however, during the autumn." — *Obs. Med.* vi. 5 (1).

Measles.

In its chief undulations, the measles curve seems to rise with the fall of the temperature, and *vice versâ*; and the influence of this element is best marked when it is above or below the forty-three years' average.

These two propositions will be proved by the following observations.

In the spring 1857 the largest number of cases occurs in the week ending May 2*nd*, when the temperature reaches its minimum (42°) this season, and when moreover it is 7·5° below the forty-three years' average. The disease curve then gradually declines as the temperature rises until August, when there is a sudden temporary increase in the number of cases (of whooping cough also), and a considerable fall from the July temperature, although the latter is still above the average, and remains so throughout the autumn. After this temporary deviation the temperature rises to 68° (7° above the average), and the number of cases diminishes again, but continues to do so for a fortnight after the temperature begins to fall.

From September 12*th* the disease curve rises gradually

maturity as the year advances; with the decline of the year it declines also. Finally the frosts of winter transform the atmosphere into a state unpropitious to its existence. — Sydenham, *Medical Observations*, iii. 2 (5).

while the temperature falls, and it continues to rise until November *7th*, and then falls until November *21st*, as though checked for a time by the temporary rise in the temperature of the preceding week (when it is 8° above the average). The disease attains its maximum in the week ending March *13th*,* the temperature having reached its lowest point in the week preceding, and being moreover 7·5° below the average.

The temperature now rises, and the disease diminishes in prevalence during the month of April: the week ending April *10th* alone has a temperature below the average, and the number of cases again slightly increases for that one week.

During the month of May the disease increases in prevalence, although accompanied by an advancing temperature which, however, is below the average; but after the week ending June *5th*, which has a mean temperature no less than 10° above the average, the disease curve gradually declines until July *10th*. The temperature in the week ending July *10th* sinks to 56° (6° below the average), and from July *24th* to August *7th* it is 2·5° below the average; and in all these instances a slight rise in the number of cases follows. With these exceptions, however, the disease curve rapidly declines until the week ending October *2nd*; this week the temperature sinks to 51° (slightly below the average), and the depression is immediately succeeded by a rapid increase in the number of cases.†

Comparisons of the daily mean barometer readings, during the period April 1857 to October 1858, tend to show that during the time this disease was most prevalent

* "They begin as soon as January; they increase gradually; they reach their height about the *24th* of March; they then gradually decline, so that, with the exception of a few that may attack isolated individuals, they disappear by midsummer." — Sydenham, *Med. Obs.* i. 3.

† Dr. Mühry states that measles in the temperate zone experiences no change with the temperature.

the fluctuations in the atmospheric pressure were far greater than when it was less rife.

Measles seem to be much influenced by the same conditions as whooping cough, since it is usually most prevalent during the same seasons; and yet it is evident that this relation is not exact, since in many of the lesser undulations of the measles-curve the variations take place in the opposite direction to those of whooping cough. (This is the case in twenty-nine out of the seventy-five weeks noticed, about 38 per cent.)

On comparing the curve from April 1857 to March 1858 with the degree of humidity, it seemed that this element had some effect upon the lesser undulations of the disease-curve, since the number of cases rose and fell with the humidity in 72 per cent of the weeks; but on comparing the second period, from April 1858 to October 1858, this hypothesis is not borne out, and the coincidence may be accidental, since in one half the weeks the variations went with the degree of humidity, in the other half they went in the opposite direction. Moreover, although in October, while the degree of humidity is rapidly rising, the disease prevails very greatly, yet in March 1858, when the number of cases reaches its maximum, the degree of humidity is very low; and in September and October 1858, when the relative amount of moisture in the air is the greatest, the number of cases is at its minimum.

Whooping Cough

Seems to be much influenced by the extremes of heat and cold, the curve, on the whole, rising with the fall and sinking with the rise of temperature.

The disease remains apparently unaffected by the gradually increasing warmth of the spring of 1857, but a decided diminution of the number of cases follows as soon as the mean temperature of the week rises to 67°, which takes place in the week ending June *27th* 1857.

From June 27*th* until October 3*rd* the temperature remains high (above the forty-three years' average nearly every week), and during this time the disease is at its minimum (between forty and fifty cases per week).

The number of cases does not again increase until the sudden fall of temperature in October and November, after which the weekly average remains pretty constant until February 13*th*, unaffected by the great fall of temperature in the week ending January 9*th* 1858, but it again rises rapidly after the extreme cold of February and March (which was much below the forty-three years average).

It remains very prevalent during the spring of 1858, but the remarkably warm June appears to check its progress, just as it did in the preceding year.

It is important also to notice that an increase in the number of cases again takes place in July, the temperature being much below the average; the year before the curve declined much more regularly and continuously.

During the summer of 1858 the disease remains almost stationary, as in the preceding year; but while it may be observed that the temperature is never so high as then, the number of cases never sinks so low (seldom below sixty).*

SCARLATINA.

A large amount of aqueous vapour in the air appears greatly to facilitate the formation and action of the peculiar scarlatinal poison, especially when this is accompanied by sudden fluctuations in the atmospheric pressure as shown by the barometer; a diminished pressure being favourable to the disease.

It is rather difficult to separate the influence of tempera-

* Drs. Donnelly and Wilde remark that spring affords rather more than the average amount of small pox, measles, scarlatina, and whooping cough. *Census of Ireland for* 1851.

ture from that of humidity, but a moderately low temperature seems to be favourable to the progress of the disease, whilst the extremes of both heat and cold seem often to exert a disturbing influence one way or the other; a temperature above the average generally diminishing, cold increasing the number of cases.

From May *9th* to August *8th* 1857, the degree of humidity remains low (below 0·7), although fluctuating considerably, and the number of cases is small; but in the lesser fluctuations the two curves rise and fall together in a remarkable manner. In the seventeen weeks from April 11 to August 8 1857, there are only three exceptions to this observation; the first two exceptions occur together in the weeks ending May 23*rd* and 30*th*, the number of cases increasing while the degree of humidity falls, and it may be noticed that the first decided rise in the temperature occurs in the preceding week; the mean temperature then rose from 45° to 56°, and during that week and the next it remained nearly 6° above the average — the barometer regularly descending for three weeks.

The second exception is in the week ending June 27*th*, and at this time again the perturbing influence of heat seems to act, the mean temperature rises 7°, and is 7° above the average. The humidity increases, and the barometer goes down, but the number of cases diminishes.

On July 18*th* the scarlatina-curve begins to rise, and on the whole continues to do so until October 31*st*, thus accompanying very closely the degree of humidity; but in the week ending August 29*th* there is a sudden fall both in the degree of humidity and in the number of cases, the mean temperature being very high (68°) and 7° above the average.

From August 29*th* there is a steady rise in the number of cases until October 3*rd*, but the following week a slight

fall occurs, and it may be noticed that in the two preceding weeks there has also been a slight fall in the degree of humidity, and in the week ending October 10*th* there is a great diminution of atmospheric pressure (barometer $29{\cdot}4^{ms}$, the lowest this year), and this is at once followed by a further rise in the degree of humidity and the number of cases.

From October 10*th* until November 21*st*, both the curves remain high, but in their secondary undulations, instead of being in accord, they supplement one another.

In the week ending November 14*th*, the highest degree of humidity accompanies a decline in the disease curve, but is followed in the week after by an increase in the number of cases. The two curves then decline on the whole until December 5*th*, when the returns of disease are discontinued for six weeks.

In the spring of 1858 the degree of humidity remains tolerably high, without any great prevalence of the disease ; but here again may be noticed for twelve weeks an almost exact accord between the rise and fall of the secondary waves of the two curves. There are two exceptions to this rule: First, in the week ending March 6, the number of cases continues to fall after the sudden depression of the degree of humidity has ceased. In this week the atmospheric pressure is again very small ($29{\cdot}6^{ms}$), and the week following there is again a sudden rise in both the disease and the humidity curve.

In the week ending March 20*th* there is no material change in the degree of humidity, but the mean temperature rises 13°, and is 6° above the average, and the scarlatina-curve descends again.

During April the number of cases diminishes gradually, and on the whole the humidity-curve declines, but fluctuates remarkably, the scarlatina-curve marking these fluctuations by slighter variations in accordance with them.

An apparent exception to the rule which we have hitherto noted now takes place. From May *8th* the disease-curve begins on the whole to rise, while the degree of humidity with great fluctuations seems to descend until the middle of June (as in the preceding year the secondary undulations corresponding with those of the disease). At the same time, however, it must be noticed that the mean temperature in the beginning of May is very low (46° or 6° below the average), and it does not rise materially until the week ending June 5.

For a fortnight after this date the temperature rises, and remains very high (66°), nearly 10° above the average, while the number of cases diminishes during the same time.

From June 26*th* the humidity and disease curves on the whole rise until October 23; but from July 10 to July 24 the degree of humidity falls as the disease curve rises; and here again we may perhaps trace the disturbing influence of temperature, the week ending July 10*th* having a a mean temperature of 56·5° (6° below the average).

In the week ending August *7th* the disease-curve rises very rapidly (sixty cases), while the degree of humidity remains low; but the preceding week the mean temperature has been 2·5° below the average.

During the four following weeks the variations in temperature would seem to have the chief influence upon the disease, the rise and fall of the fluctuations of temperature and scarlatina supplementing one another very closely.

In the week ending September 4, the barometer is very low, and the following week the degree of humidity rises considerably, while the temperature remains stationary, but there is a rapid rise in the disease curve.

The number of cases again falls greatly in the week ending September 18, probably from the action of the unusual heat, the temperature rising to 63·5° (6·5° above

the average), while the degree of humidity continues to rise.

From this time, however, until October 16*th*, the humidity again appears to exert its influence, and the curves are in accordance.

The disease curve reaches its highest point for the year (200 cases) in the week ending October 16*th*, the degree of humidity rising rapidly until October 23*rd*, but the temperature not descending much, and remaining 2° above the average.

It is interesting to observe the manner in which the curve of scarlatina supplements the curves of whooping cough and measles. "Thus they vex humanity by turns, as the constitution of the year and the sensible temperature of the air most assist the one or the other." — Sydenham.*

In the foregoing examination into the effects of the several meteorological elements upon scarlatina, it will be seen that we have ascribed to humidity the chief influence, but at the same time have carefully noted the effects of variations of temperature and pressure of the atmosphere; but it may be that we have not sufficiently indicated the reasons for our opinion.

Without very close comparison it would be very difficult to decide whether temperature or humidity had the greatest influence upon this disease. First, if we take the correspondence of the curves during the same times, we shall find that in 64 per cent of the weekly periods the number of cases rose and fell with the fall and rise of the thermometer, and in 63 per cent with the rise and fall of the degree of humidity; in 42 per cent of the periods

* Sydenham states that scarlet fever may appear at any season, but oftenest towards the end of summer. — *Med. Obs.* vi. 2, 1. He also speaks of one epidemic being driven out by another "ut clavum a clavo." — *Med. Obs.* ix. 1, 7.

these two elements might act together, the temperature falling as the degree of humidity and disease-curve rise, and *vice versâ*. Of the weeks in which the degree of humidity and temperature rise and fall together, the apparent effects, as shown in the rise and fall of the disease-curve, are almost exactly balanced, there being fifteen points of agreement with the temperature, sixteen with the humidity-curve. The fact of accordance between the rise and fall of the curves, however, must be of little importance in determining the influence of the element upon the disease, compared with observations upon the *actual state* of the air at the time of prevalence or absence of the disease.

A few instances will, we think, show that the temperature, although by no means inactive, exerts less influence than the humidity.

While the degree of humidity is at its lowest point in 1857, between June 6 and July 18, the number of cases is also the least, scarcely rising above thirty. During the corresponding period in 1858, between April 1 and July 10, although the disease is rather more prevalent than in the year before, yet the number of cases rarely exceeds fifty, and does not increase until the degree of humidity begins on the whole to rise.

Both in 1857 and 1858, when the amount of moisture in the air is greatest, the disease-curve is at its highest point.

On the other hand, a low degree of temperature accompanies both the smallest and largest number of cases in both 1857 and 1858, and the same is true of a high temperature; *e.g.* on July 18, 1857, the mean temperature is 68° while the disease is at twenty-five, and in August 1858, with the mean temperature above 60°, the number of cases remains above 100. Notwithstanding this remark, however, many of our observations will prove that temperature has an important modifying action.

Thus far has our investigation carried us. We already trace, though often but faintly, the influence of the few meteorological elements which we have studied. Taken in connection with large numbers of cases of disease, although a clearly defined and exact accordance cannot be found, still we perceive a certain general relation existing between them, and enough may perhaps have been done to prove the value of such an inquiry. The returns for several of the weeks, especially those ending August 22*nd* and 29*th*, 1857, and March 6*th*, 1858, are very imperfect; but after careful examination we do not find that these deficiencies affect the conclusions at which we have arrived. In each instance, when appreciable, the probable amount of error arising from this source had been marked in pencil upon the chart.

Before closing this paper we must state that it seems very desirable that many other branches of meteorological research should be included in the inquiry, and compared with disease; among these may be mentioned winds, electricity of the atmosphere, rain, microscopical and chemical analyses of the atmosphere. Under the last-named head regular series of observations, made at various stations with the sepometer of Dr. R. A. Smith, F.R.S., would be of very great importance.

XI. — *On the Phenomena of Groups of Solar Spots.*

By Joseph Baxendell, F.R.A.S.

Read before the Physical and Mathematical Section,
October 13th, 1859.

It is well known that the spots on the sun generally occur in groups, which differ considerably in size and form, and in the duration of their appearance; and which frequently undergo great changes in short intervals of time. Although they have long engaged the attention of astronomers, little progress has yet been made in the investigation of the laws which regulate their formation and their diversified appearances and changes. It has long been known that they occur principally in two bands or zones parallel to the solar equator, and that they are rarely seen beyond the limits of 35° north and south latitude; and Mr. Carrington has shown that the position in latitude of these zones is subject to considerable and rather sudden changes. M. Schwabe's long series of observations has proved that there is a pretty regular period of increase and decrease in the number of the solar spots. Mr. Dawes and Professor Secchi have observed that large spots have sometimes a movement of rotation; and Dr. C. H. F. Peters and Mr. Carrington have pointed out that the individual spots of small groups have a tendency to diverge or recede from each other. There are, however, certain phenomena of groups of spots which, so far as I am aware, have not

been alluded to in any astronomical work or memoir, but which seem to me to merit attention, as indicating the direction in which future inquiries ought to be made; and I have therefore thought that a brief notice of them might not be unacceptable to the members of this section.

The first point to which I would draw attention is the fact that in those groups, which consist of spots differing considerably in size, the largest spot is generally in the preceding part of the group. An examination of such published drawings and descriptions of the solar spots by other observers as have fallen under my notice has confirmed the conclusion drawn from my own observations; and in a series of unpublished diagrams of the solar spots made by my friend Mr. Williamson of Cheetham Hill, in the years 1849, 1851, and during the present year, and which he has kindly allowed me to inspect, I find that out of thirty-one different groups only *four* had the largest spot in the following part of the group, the ratio of exceptions to the rule being therefore about one to seven. My own observations indicate a much smaller ratio, and I must remark that as Mr. Williamson's principal object was to show the positions of the groups on the solar disc and not to delineate very exactly their details, many of the groups laid down in his diagrams do not exhibit any very decided difference of size in their components, and are therefore not available for the purposes of this branch of the inquiry.

The next feature in the constitution of groups of spots to which I have to allude is, that a great number of these groups may be regarded as consisting of two *sub-groups*, each of which generally contains one or two spots decidedly larger than the rest. In their early stages these sub-groups have often no apparent connexion, but after a certain time small spots break out in the interval between them, and form an irregular line or band which completes

the group. Some of these new spots occasionally increase in size until they equal or even exceed the principal spots of the sub-groups. In such cases, however, the latter generally diminish, and the whole group undergoes considerable changes; but amidst all these changes there appears to be a constant tendency of the forces which produce the spots to concentrate in *two* places in the group at some considerable distance apart, and lying in a direction which is evidently more or less dependent upon that of the sun's rotation; and it often happens that groups which have apparently quite lost their original binary character again resume it before their final decay and extinction.

The preceding sub-group of binary groups is almost invariably the first to appear, and generally the last to disappear, the exceptions in the first case being fewer than in the latter.

Mr. Williamson's diagrams have proved very valuable in affording many cases of groups which exhibit a decidedly binary character; and from the descriptions of the solar spots given by former observers there can be little doubt that groups of this class have at all times been of common occurrence. Thus I may quote as an instance an observation by Sir William Herschel, which will be found recorded at p. 272 of the *Phil. Trans.* for 1801, and which is an excellent illustration of one of the most interesting phases of these binary groups. It is as follows:

"January 18, 1801. Between two clusters of openings near each other, there are some, as I suspect, incipient openings: they resemble coarse pores of indentations. January 19, 1801. The incipient openings between the clusters of yesterday's observations are completely turned into considerable openings. It seems as if an elastic but not luminous gas had come up through the pores or incipient openings, and spread itself on the luminous clouds, forcing them out of the way, and widening its passage."

Since I first satisfied myself as to the reality of the binary character of groups, I have found that in the case of spots apparently single and isolated a careful examination with a larger aperture and higher power will often reveal a minute spot at a distance on the following side, which afterwards frequently proves to be the nucleus of a following sub-group; and though I formerly regarded many spots as single and isolated, I now believe that, in fact, such spots are of extremely rare occurrence.

The two centres of force or activity in binary groups are sometimes very widely separated, instances not uncommonly occurring in which the distance between them exceeds ninety thousand miles.

With regard to groups which exhibit anomalous appearances and curious and complicated changes, I have sometimes observed that they arise from the interference of two smaller groups which have broken out near each other, and have gradually extended themselves until they formed one compound group. I need not remark upon the importance of a careful observation of the phenomena of these compound groups.

I have alluded to the observations of Mr. Dawes and Professor Secchi on the rotation of spots, and I may add that in all the cases of rotation which have come under my own notice the rotating spot has been the principal member of the *preceding* sub-group; but I must remark that the observations which I have hitherto made are very far from being conclusive as to whether this rotation is real or only apparent.

It will not escape remark with reference to the binary character of groups, that there is considerable difficulty in conceiving the mode of operation by which the forces that produce the spots in a group should first develope themselves, almost simultaneously, at two points so widely distant from each other; and I must admit that in the

present state of the inquiry I am not prepared to enter into any theoretical explanation of this remarkable phenomenon. My observations have hitherto been made without reference to any particular theory, and they have led me to believe that much yet remains to be done in accumulating the raw materials of observation before we can obtain results sufficiently well established to form the basis of a philosophical theory of solar phenomena; and at present I do not offer the conclusions given in this communication as anything more than the first results of an attempt to pursue, without reference to any theory, a systematic examination of the phenomena of the solar spots.

The following extracts from my journal of observations will serve to illustrate the several points to which I have drawn attention. Greek and Roman letters are used to denote the different spots and groups as laid down in diagrams in the journal; and I have often found it convenient to indicate the *size* of spots by reference to a scale of magnitudes, in which the first magnitude represents a spot of the largest size and the tenth one of the smallest. The telescopes which have been used in the observations are a small but excellent achromatic by Mr. Dancer of twenty-two inches focal length and 2·6 inches aperture; a Newtonian reflector of eighty inches focal length and seven inches aperture; and the excellent equatorially-mounted achromatic of Mr. Worthington's observatory, which has a focal length of seventy inches and an aperture of five inches.

Observations.

September 9, 1859, 9*h.* a.m. Of the *six* groups now visible on the sun's disc, *five* have their largest spots in the preceding part of the group, and the sixth consists of spots differing little in size.

September 22, 5*h.* p.m. The sun has gone through half a revolution since the observation of the 9*th* instant,

and in all the *six* groups now visible the largest spot is in the preceding sub-group.

September 15, 4*h.* 15*m.* p.m. All the groups now visible have an elongated form, and in *five* of them the largest spot is in the preceding, and the next in size is in the following end of the group, giving each the appearance of a double group, the sub-groups being connected by irregular lines of very small spots. In the sixth group (H) the two largest spots are equal or nearly equal in size, but there is a disposition to the same arrangement.

September 16, 8½*h.* a.m. Preceding large spot of H now decidedly larger than following; preceding 7, and following 8 magnitude.

September 17, 9*h.* a.m. A new group has broken out at ϵ; following principal spot 7*th* mag.; preceding 7½*m.*; one or two 10*th* mag. between them; the following principal spot is very black, and has no penumbra. The chief spot in following sub-group of H is also very black. 5*h.* p.m. Three or four small spots *n f* following the sub-group of H. Preceding large spot of ϵ is now rather larger and blacker than following large spot; four spots in preceding sub-group.

September 18, 11*h.* a.m. Following large spot of H now larger than preceding.

September 19, 9*h.* a.m. Many new spots in H, and several in ϵ, but second principal spot of H and both those of ϵ have diminished in size. A new group has entered disc at J — principal spot 7½ mag., second principal 9 mag., and three or four 10*th* mag. spots. A new group has broken out since yesterday at θ; the following spot is the largest, and is very black; two principal spots and many smaller ones. 4¾*h.* p.m. The spots in ϵ are now all nearly of the same size. Counted twenty-five spots in group H. Preceding principal spot of θ is now equal to the following, and two spots have broken out near the latter.

September 21, $3\frac{1}{2}h$. p.m. The spots in θ have increased in size and number. The spots in J are larger, and the principal of following sub-group is rather larger than that of preceding sub-group. The spots in ϵ and H have changed considerably, and the leaders in both are again decidedly larger than any of the other spots in the groups.

September 22, $9h$. a.m. There seems to have been a movement of rotation in preceding large spot of J since yesterday afternoon.

September 24, $9h$. a.m. Preceding large spot of J appears to have gone round three quarters of a revolution since $9h$. a.m. September 22*nd*. The rotation, if real, has been in the direction E.N.W.S. as seen from the earth, or to a spectator on the surface of the sun E.S.W.N., the spot being in the *northern* hemisphere. There seems to have been little or no movement in the large spot of following sub-group.

September 25, $11h$. a.m. θ approaching western limb, and large spot of preceding sub-group is now much larger than any other in the group. The following sub-group contains more spots than the preceding. The position of the longer axis of principal spot of J seems to have altered again, and in the same direction. A 9*th* mag. spot has broken out at ξ, far removed from any other spot. $3\frac{1}{2}h$. p.m. The distance between the centres of the principal spots of the sub-groups of J is $3' \ 29.4''$, or more than 96,000 miles.

September 26, $5h$. p.m. Preceding large spot of J appears to have rotated through an angle of about 30° since yesterday forenoon, and in the same direction as before. The large spot in following sub-group seems to have moved very little, and if at all it has been in a contrary direction. The spot ξ has increased, and I am pretty sure I caught sight of a minute companion at a distance on the following side.

September 27, $9\frac{1}{4}h.$ a.m. The large spot of J has rotated still further, and a small spot which was in a projecting part of its penumbra *n p* is now quite detached, with a bright line between. The following large spot seems to have changed little, if at all. ξ is now a fine double group; principal spot *6th* mag.

September 29, *9h.* a.m. Preceding spot of J presents no fresh indication of rotation. Four $8\frac{1}{2}$ or *9th* mag. spots, which I regard as two new groups o and ϕ, have broken out in the space within the groups M, N, O, and P; preceding spots in each a little larger than following.

October 1, *4h.* p.m. Many new spots in o and ϕ, and they now form one compound group, in which, however, there is a tendency to the binary arrangement.

October 3, *8h.* a.m. The compound group $(o+\phi)$ has five *8th* mag. spots; the rest are *9th* and *10th* mag. It has a very irregular form.

October 5, $8\frac{1}{2}h.$ a.m. Many of the spots in following part of the compound group $(o+\phi)$ have united into *one*, which is a *5th* or *6th* mag. in size.

October 6, $4\frac{1}{2}h.$ p.m. The compound group $(o+\phi)$ now consists of a *5th* mag. spot with a very large penumbra, preceded by several smaller spots.

There is a common, and, as it appears to me, very significant feature of binary groups which is often alluded to in the journal, and I will therefore add an observation having reference to it.

September 16, 1859, $8\frac{1}{2}h.$ a.m. The following subgroups of D, F, G, and H contain more spots and extend over a larger area than the preceding; but the principal spots in the preceding sub-groups are larger than those in the following.

XII. — *On the Rotation of Jupiter.*

By Joseph Baxendell, F.R.A.S.

Read before the Physical and Mathematical Section,
December 8th, 1859.

During the last winter and spring I availed myself of the opportunity presented by the appearance of several small isolated dark spots on the surface of Jupiter to make a series of observations with the ultimate view of obtaining a re-determination of this planet's period of rotation; and with the more immediate object of testing the conclusions which have been drawn from the observations of Cassini, Maraldi, Sir William Herschel, Schroeter, &c., to the effect that different spots have different periods of rotation; that the time of rotation of the same spot will sometimes vary; and that in general spots near the equator move more quickly than those in higher latitudes.

Some of the observations were made at my own house with a Newtonian reflector of seven inches aperture and eighty inches focal length; but the greater number were made at Mr. Worthington's observatory with his excellent equatorially-mounted achromatic of five inches aperture and seventy inches focal length, and Newtonian reflector of thirteen inches aperture and nine feet focal length. The magnifying powers employed in making the estimations of the spots ranged, according to atmospherical circumstances, from 130 to 301, those most frequently used being 180 and 223.

The object of each night's observations being to determine, as accurately as possible, the time when the meridian of a spot passed through the centre of the disc of the planet, careful eye estimations of the position of the spot were taken at intervals during its transit across the disc; each result was then reduced to the central position by the aid of small tables constructed for the purpose; and the mean of all the reduced results — allowing weights in certain cases — was taken as the true time of the spot's apparent meridional conjunction with the centre of the disc. The times thus obtained were afterwards reduced to the epoch of the first observation, November *7th, 10h. 40m.* G. M. T., by applying the corrections due to the altered geocentric longitude of Jupiter, and to the changes in the value of the light equation.

Attempts were made to determine the positions of the spots by micrometrical measurement; but it was found that the results obtained in this way did not exceed in accuracy (except in the case of a very conspicuous spot and under highly favourable atmospherical circumstances) those obtained from eye estimations, or from carefully drawn diagrams; and, moreover, valuable estimations could often be made when, from frequent interruptions by rapidly passing clouds, complete micrometrical measurements were quite impracticable.

The number of spots observed was *seven,* and they were denoted in the journal by the letters A, B, C, &c., in the order in which they were first seen; but the observations of *five* only were sufficiently numerous and exact to be available for the determination of their periods of rotation They were all distributed in a narrow band or zone in the southern hemisphere, *six* of them being nearly on the same parallel of latitude, and the *seventh* only about fifteen degrees more to the south. It will be evident that this peculiarity of distribution was very unfavourable for an

investigation of the differences of velocity of rotation said to be due to differences of latitude of the spots.

The following tables contain the corrected Greenwich mean times of the observed meridional conjunctions of the spots A, B, C, E and F, with the centre of the planet's disc:

Spot A.

1858.			1859.		
Nov. 7	10	40·0	Jan. 18	10	33·0
17	9	2·8	19	6	14·6
Dec. 2	6	34·2	22	13	44·5
4	8	14·0	23	9	38·6
20	11	24·9	28	8	47·5
23	8	53·1	Feb. 2	7	55·5
1859.			7	7	9·5
Jan. 1	11	23·5	11	10	21·5
7	6	25·4	Mar. 10	7	49·6
12	5	31·6			

B.

1858.			1859.		
Dec. 20	10	33·1	Jan. 28	7	54·7
23	7	58·9	Feb. 2	7	5·2
1859.			6	10	19·4
Jan. 1	10	27·0	7	6	17·1
14	6	20·9	9	7	49·7
16	7	57·6	11	9	27·4
18	9	32·6	Mar. 10	6	51·6
22	12	50·2	29	7	27·1
23	8	42.8	31	9	11·4

C.

1859.			1859.		
Jan. 22	7	46·3	Feb. 27	7	33·8
27	6	56·2	Mar. 13	9	3·9
Feb. 10	8	37·2			

E.

1859.			1859.		
Jan. 23	6	27·3	Mar, 7	6	55·5
Feb. 6	8	2.6	Apr. 7	7	26·9
11	7	4·9	9	9	1·2
23	7	4·7	21	9	1·8

F.

1858.			1859.		
Nov. 12	11	44·2	Feb. 2	9	15·9
1859.			5	6	47·5
Jan. 28	10	16·2	10	6	3·1

Dividing the observations of each spot into two groups, finding the mean epoch for each group, and dividing the interval between the epochs by the number of included revolutions, we obtain the following values of the mean period of rotation:

	h.	m.	s.	s.
From Spot A,	9	55	46·086	± 0·439
B,	9	55	44·821	± 0·578
C,	9	55	39·240	± 1·650
E,	9	55	37·812	± 0·636
F,	9	55	39·858	± 1·650

Between the 27*th* of October 1858 and the 14*th* of January 1859, Sir William Keith Murray Bart., of Ochtertyre, made a very interesting series of observations of Jupiter with a fine Munich achromatic refractor of $6\frac{1}{3}$ inches aperture, and he kindly forwarded to me exact copies of a series of diagrams, showing the positions of some of the spots; and also extracts from his journal. These observations were, I believe, made without any special reference to the question of the period of rotation, the observer's principal object being to record and delineate any remarkable or interesting phenomena which might present themselves on the disc of the planet; but as great care has evidently been taken in laying down very exactly the positions of the spots in the diagrams, and in noting the corresponding times, I have reduced and discussed the observations of spots A and B, and have obtained the following mean periods:

	h.	m.	s.	s.
From Spot A,	9	55	45·96	± 1·33
B,	9	55	43·62	± 1·65

The close agreement between these results and those derived from my own observations will serve to show the degree of reliance which may be placed upon observations of this nature. Had Sir William Keith Murray's observations been continued a month or two longer there is no doubt the agreement would have been still closer; but unfortunately they were interrupted in consequence of

the removal of the Munich refractor to make room for a still larger instrument, a fine equatorially-mounted achromatic of thirteen feet focal length and nine inches aperture, by Cooke, of York.

Reverting to the results of my own observations, the periods of spots B, C, E and F, compared with that of A, give the following differences and probable errors:

	Difference.	*Probable Error.*
A and B	1·265	± 0·719
C	6·846	± 1·477
E	8·274	± 0·760
F	6·228	± 1·477

As some of these differences considerably exceed their probable errors, the conclusion which has been drawn from former observations, that different spots have different periods of rotation, is thus fully confirmed.

The following table shows the observed distance between the spots A and B on eleven different nights:

1858.	*m.*	1859.	*m.*	1859.	*m.*
Dec. 20	51·8	Jan. 22	55·3	Feb. 7	52·4
23	54·2	23	55·8	11	54·1
		21	52·8		
1859.					
Jan. 1	56·5	Feb. 2	50·3	Mar. 10	58·0
18	60·4				

The probable error of a single distance is 2·7 *m.*, and we may therefore fairly conclude that the changes indicated by the numbers in the table were real, and that the distance between the spots was greater on the 18*th* January and 10*th* of March than on the 20*th* December and 2*nd* of February. We have already seen that the probable error of the mean period of B, as derived both from my own and Sir William Keith Murray's observations, is greater than that of A, and it may therefore be reasonably inferred that the changes in the distance between the spots were principally due to irregularities in the motion of B. The few observations which were made of spot F show that its motion also was not uniform. On the 27*th* of

October, according to Sir William Keith Murray's observations, it followed A 1*h*. 55·8*m*.; on the 12*th* November, a tolerably good observation of my own gave precisely the same difference; but on the 28*th* January the distance had diminished to 1*h*. 41·4*m*., and on the 2*nd* February it was only 1*h*. 32·7*m*.

Adopting the position of the axis of rotation of Jupiter given in the introduction to Damoiseau's *Tables Ecliptiques des Satellites de Jupiter*, the mean result of three nights' micrometrical measures gives the latitude of spot A = 13° 47′ south; and the mean deduced from *fourteen* of Sir William Keith Murray's diagrams is 13° 11′ S. The spots B, C, and F were all very nearly on the parallel of A, while E, the spot which had the shortest period of rotation, was in latitude about 28½° S. It appears, therefore, that the conclusion drawn by Cassini, from his own observations, that spots near the equator of the planet generally move quicker than those in higher latitudes, receives no support from these observations.

It is hardly necessary to remark that the results now given afford no certain information as to the period of rotation of the planet itself, as distinguished from that of its spots. On the contrary they seem to me to indicate very clearly — especially when taken in connexion with the results obtained by former observers — that in the present state of our knowledge of the phenomena which take place on the surface of the planet or in its atmosphere, any conclusion as to its exact period of rotation based upon observations of the times of rotation of its spots must necessarily be very precarious.

XIII. — *On the Estimation of Sugar in Diabetic Urine by the loss of Density after Fermentation.*

By William Roberts, M.D. Lond., *Physician to the Manchester Royal Infirmary.*

Read October 16th, 1860.

When saccharine urine is fermented with yeast its specific gravity, previously ranging from 1030 to 1050, falls to 1009 or 1002, or even below 1000. This result is chiefly due to the destruction of the sugar it contained, but partly also to the generation of alcohol, and its presence in the fermented product.

As the diminution of density must be proportional to the quantity of sugar broken up by the ferment, the amount of this diminution evidently supplies a means of calculating how much sugar any urine contains, always provided that the remaining ingredients of the urine continue unchanged, or become changed in some uniform ratio.

In order to ascertain the exact relation subsisting between the density lost on fermentation and the sugar destroyed, experiments were made on the fresh urine of several diabetic patients in the Royal Infirmary.

The following procedure was adopted:

1. The amount of sugar per 100 parts was first accurately determined by the volumetrical method, with Fehling's test solution.

2. Next the density was taken by the specific gravity bottle.

3. About 4 *oz.* of the urine were then placed in a 12 *oz.* bottle, with a drachm or two of German yeast, and set aside in a warm place to ferment, taking care to cover the mouth of the bottle with a slip of glass or a loose cork.

4. In from twelve to eighteen hours fermentation was usually over, and at the end of twenty-four hours the froth and scum had subsided or been dissipated sufficiently to permit the density to be again taken.

Operating in this way on a urine passed on the 21*st* of April, I obtained the following results:

Sugar per 100 parts by the volumetrical method ...	7·69
Density before fermentation at 60° or D...............	1038·60
Density after fermentation at 60° or D′...............	1005·92
Density lost or D – D′	32·68

The relation, therefore, between the density lost and the per centage of sugar in this instance was as 32·68 to 7·69, or as 1 to 0.235; so that by multiplying the density lost into the co-efficient 0·235 we have for product the amount of sugar per 100 parts which this urine contained. That is, sugar per 100 parts or $S = (D - D') \times 0{\cdot}235$.

On repeating the experiment a great number of times with different specimens of urine and different specimens of yeast, the number 0·230 was found to be the more exact co-efficient.

The degree of exactitude with which the quantity of sugar may be determined by this method is very great; indeed with the precautions to be mentioned presently it seems susceptible of nearly as much accuracy as the volumetrical method.

The following table places in comparison twenty observations made by the two methods on various diabetic urines, with densities ranging from 1031·52 to 1053·48:

TABLE I.

No.	Sugar per 100 parts by the formula $S = (D - D') \times 0.23$.	Sugar per 100 parts by direct volumetrical analysis.	Difference.
1	7·51	7·69	0·18
*2	7·47	7·69	0·22
3	6·68	6·66	0·02
*4	6·72	6·66	0·06
5	5·16	5·18	0·02
*6	5·19	5·18	0·01
7	5·65	5·77	0·12
*8	5·65	5·77	0·12
9	4·47	4·35	0·12
*10	4·49	4·35	0·14
11	7·85	8·06	0·21
12	5·91	6·10	0·19
13	11·27	11·36	0·09
*14	11·21	11·36	0·15
15	5·69	5·68	0·01
16	8·11	8·06	0·05
*17	8·09	8·06	0·03
*18	8·00	8·06	0·06
19	8·29	8·20	0·09
20	7·61	7·74	0·13

These results are so close that the discrepancies may be regarded as due to errors of manipulation rather than to any fault in the method.

In pursuing the inquiry further it was found that the volumetrical analysis, in spite of every care in its performance, did not possess the delicacy and certainty required in a standard when minute differences were concerned; insomuch that, when a discrepancy appeared between the indications of the volumetrical and the fermentation methods, it was found impossible to decide in which proceeding the error lay.

In order therefore still further to test the constancy of the results, artificial diabetic urines were prepared by diluting a natural diabetic urine with known volumes of water, or of a healthy non-saccharine urine. Assuming the estimate of the sugar in the original urine to be correct, the quantity of sugar in the dilutions could be ascertained

* Those marked with an asterisk are duplicate experiments.

with almost absolute accuracy by a simple calculation. In the following table the results obtained by fermenting these dilutions are placed side by side with the calculated quantity of sugar.

TABLE II.

No.		Sugar per 100 parts according to the formula S=(D-D′)×0·23.	Sugar per 100 parts by calculation from the first estimate.	Difference.
1	A natural diabetic urine	5·91	—	—
2	The same mixed with $\frac{1}{10}$ of its bulk of water	5·31	5·32	0·01
3	The same mixed with $\frac{2}{10}$ of its bulk of water	4·71	4·73	0·02
4	The same mixed with $\frac{3}{10}$ of its bulk of water	4·16	4·14	0·02
5	The same mixed with $\frac{1}{10}$ of its bulk of healthy urine	5·34	5·32	0·02
6	The same mixed with $\frac{2}{10}$ of its bulk of healthy urine	4·77	4·73	0·04
7	The same mixed with $\frac{3}{10}$ of its bulk of healthy urine	4·15	4·14	0·01
8	The same mixed with $\frac{9}{10}$ of its bulk of healthy urine	0·70	0·59	0·11

Numbers so nearly alike as those in these two columns may be considered as practically identical. In the last experiment only, where the quantity of sugar was under one per cent., was there a sensible discrepancy.

Satisfied now with the accuracy of the fermentation method, I was desirous of determining with more certainty and exactitude the required co-efficient; which, from the preceding experiments, using the volumetrical analysis as a standard, was fixed at 0·23.

I was unable to obtain grape sugar in sufficient purity to make solutions of known strength, and had recourse to cane sugar; using for the purpose the best loaf sugar of the shops.

Solutions were made, varying in strength from 2 to 10 per cent. both with distilled water and healthy urine. But as cane sugar becomes converted into grape sugar under

the influence of yeast before fermentation begins, and the density of the solution is thereby materially increased, corrections had to be made on both these accounts before the experiments could be fairly compared with those on diabetic urine. In making the first correction cane sugar was taken as C_{12} H_{11} O_{11} and grape sugar as C_{12} H_{12} O_{12}. In making the second correction allowance was made for the increase of density in accordance with the tables published by the authors of the report on "Original Gravities." *

Six solutions were made of cane sugar in water; two containing 10 per cent. and the remaining four containing respectively 8, 6, 4 and 2 per cent. of sugar.

The mean co-efficient obtained from these six experiments was 0·234.

Twelve solutions were similarly made in a healthy non-saccharine urine; two containing 10 per cent., three 8 per cent., and two 2 per cent. The remainder contained respectively 6, 4, 1·4, 1 and 0·6 per cent. These yielded a mean co-efficient of 0·228; and the general mean for the eighteen trials was a fraction under 0·23. These experiments therefore confirmed the results previously obtained with the volumetrical analysis as a standard.

Equally correct results were obtained in operating on weak solutions, containing only 1 or 0·6 per cent. of sugar, where the density lost was not more than 3 or 4 degrees, as with solutions containing 10 per cent. of sugar, in which the loss of density exceeded 43 degrees.

Having examined the question experimentally, and fixed the co-efficient from multiplied trials under varied conditions, it was not without interest to examine a little more closely the several items which go to constitute the "density lost" in fermentation, and to endeavour to arrive

* Report on Original Gravities, by Professors Graham, Hofmann and Redwood, in the *Quarterly Journal of the Chemical Society*, 1853.

synthetically at similar results to those obtained by the more empirical method of direct experiment.

If we take a solution of cane sugar in water, made so that 100 grains are contained in each 1000 grain-measures, its density at 60° will be 1038·64.

Such a solution consists by weight of 100 grains of sugar, and 938·64 grains of water.

After fermentation there remains 53·74 grains of alcohol, and 987·18 grains of water.

Such a mixture of alcohol and water has, according to Gilpin's tables,* a density of 993.

This figure is very near that obtained in actual fermentation. The density of the fermented product is, however, a little higher, about 1½ or 2 degrees. This slight excess is to be attributed to the escape of a portion of alcohol with the carbonic acid during fermentation, and to a little soluble matter taken up from the yeast.

Some effect probably, though I know not of what kind, is likewise produced by the retention in the liquor of more than its own bulk of gaseous carbonic acid in a state of solution.

It was uniformly found, with solutions of equal strength, that those made with urine showed rather more "density lost" than those made with water. This is an indication either that changes take place in some of the non-saccharine constituents of the urine during fermentation — but changes so slight and so constant in their nature that they do not interfere materially with the accuracy of the mode here proposed for estimating sugar — or else that urines usually reputed non-saccharine do in reality contain a small quantity of sugar.

An excessive quantity of yeast was used in these experiments in order to hasten the process of fermentation. The quantity of soluble matter taken up from the yeast was

* See Henry's *Chemistry*, tenth edition, vol. ii. p. 343.

very small. Two drachms shaken up with 4 *oz.* of water, and left in a warm place for thirty-six hours, only increased the density of the water by 0·3°. In the above experiments a piece of yeast about the size of a large filbert or small walnut was employed to ferment four ounces of urine, a little more or a little less making scarcely a sensible difference in the results.

The fermented urine continues somewhat turbid for a day or two after fermentation is completed, and the degree of this turbidity has an appreciable though slight effect on the density. In the experiments detailed in this paper the second density was taken about twenty-four hours after the addition of yeast, and before the urine had completely cleared. It was found in four trials that if the urine was allowed to rest twelve hours longer the density fell by 0·2° or 0·3°. It is desirable, therefore, where a series of experiments is made, and scrupulous accuracy and uniformity are required, always to take the second density at about the same period after fermentation has ceased.

The method here proposed for determining sugar is not put forward in rivalry with the accurate and elegant volumetrical process now usually employed by chemists; but it is believed that it will be of great service to the medical practitioner, who is unaccustomed to delicate chemical manipulations. Any one who has had much practice in the volumetrical method knows that great nicety and considerable experience are requisite to insure trustworthy results. Fehling's test solution also is liable to speedy deterioration unless hermetically sealed from the air. This arises from the conversion of a portion of the tartaric acid of the test into racemic acid, which, equally with sugar, has a reducing power on the oxide of copper, and when present of course vitiates the results. For these reasons it may be surely predicted that the volumetrical method can never come into general use at the bed-side.

The fermentation method, on the other hand, is of exceedingly easy performance, and the taking of densities is an operation to which the medical practitioner is daily accustomed. I shall have an opportunity through another channel to bring this method under the notice of my medical brethren, and to enter more fully on the particulars which concern its clinical application. I content myself here with a short sketch of the process, as it may be conveniently carried out in private or hospital practice.

1. The specific gravity of the urine is taken at the ordinary temperature of the ward or bed-room.

2. Three or four ounces of the urine are poured into a 12 *oz.* phial, together with a lump of German yeast of the size of a large filbert.

3. The bottle is lightly corked, or covered with a slip of glass, and set aside in a warm place to ferment.

4. In about eighteen hours, when the fermentation has entirely ceased, the bottle is tightly corked and removed to the ward or bed-room so that it may cool to the temperature at which the specific gravity was taken the day before.

5. The urine in the meantime clears, and in five or six hours it may be decanted into an appropriate vessel and the specific gravity taken again.

6. The amount of "density lost" is thus ascertained, and the following simple and most convenient rule expresses the result of the analysis. *Each degree of "density lost" indicates one grain of sugar per fluid ounce of urine.* So that in the example already quoted in the earlier part of this paper, where the urine lost 32·68° of density after fermentation, the quantity of sugar indicated was 32·68 grains per ounce, or 653·6 grains per imperial pint.

XIV. — *On the alleged practice of Arsenic-Eating in Styria.*

By Henry Enfield Roscoe, B.A., Ph.D.,
Professor of Chemistry at Owens College, Manchester.

Read October 30th, 1860.

It has been frequently stated that among the peasants of Styria the habit prevails of regularly taking into the system large quantities, from two to five grains daily, of arsenious acid. This extraordinary practice is said to be indulged in for the purpose of improving the health, avoiding the danger of infection, and raising the whole tone of the body.

Dr. Taylor shows in his excellent treatise on poisons,* that this widely spread notion is traceable to the statements on the subject of Dr. Von Tschudi, in a paper published by him in the *Wiener Medicinische Wochenschrift* for October 11*th*, 1851. The late Professor Johnston repeats these statements of Von Tschudi's in his popular work on the *Chemistry of Common Life*, and this seems to be the source whence the assertion has gained some credence in this country. Dr. Taylor points out that Von Tschudi does not give satisfactory evidence in proof of this singular and very improbable statement, inasmuch as he did not either submit the so called

* Taylor *On Poisons*, second edition, 1859, p. 91, &c. Dr. Taylor states that from two to three grains of arsenious acid may be regarded as the smallest dose usually producing death.

arsenic to a chemical analysis, or examine the urine or fæces of those persons who were said to indulge in the practice of arsenic-eating; and Dr. Taylor suggests that the white powder eaten by the Styrian peasants is not arsenious acid, but probably oxide of zinc. Mr. Kesteven, in some papers published in the *Association Medical Journal* for 1856, reviews the whole subject, gives Von Tschudi's communication *in extenso*, discusses the articles upon the subject by Mr. Boner which appeared in *Chambers's Journal*, protests against the light manner in which Johnston adopts and theorises upon the unproved statements of Von Tschudi, and concludes that the story of the Styrian arsenic eaters is not only unsupported by adequate testimony, but is inconsistent, improbable, and utterly incredible. Since the publication of Mr. Kesteven's paper, Mr. Heisch has communicated in the *Journal of the Pharmaceutical Society* for May 1860, statements concerning a number of interesting and apparently well-attested cases of arsenic-eating obtained from persons of position in Styria.*

Believing that this interesting question, as to whether or not persons are able, without apparent injury, to take into their systems doses of arsenic usually supposed sufficient to produce death, could only be settled by more precise local information than is at present made public, I was supplied, through the kindness of my friend Professor Pebal of Lemberg, with a number of reports addressed by seventeen medical men in Styria to the government medical-inspector Dr. Julius Edler Von Vest of Grätz, respecting the alleged practice of arsenic-eating in that district. A notice of some of these reports, as well as of other mat-

* Another statement concerning the "Styrian Poison-Eaters" was published in a Grätz newspaper, the *Tages Post*, on March 30*th* and April 8*th*, 1860, by Dr. Schidler, in which several cases of arsenic-eating which came under his notice are detailed.

ters affecting this question, was laid before the Academy of Sciences in Vienna a short time ago by Dr. Schäfer* of Grätz. I shall have the honour of laying before the Society the results of these, as well as of other communications which I have received upon this subject.

It is almost unnecessary to say at the outset, that, upon a question of this kind, nothing but the most positive and direct evidence can be received as conclusive. My aim will simply be to endeavour to bring forward such evidence as may help to decide the question whether arsenious acid is, or is not, regularly eaten by men in Styria in quantities usually supposed sufficient to produce death. I shall not attempt to discuss or explain the superstitious notions with which many of the reports naturally enough abound; nor shall I detail the various purposes for which the arsenic is said to be taken, or the various modes in which the dose is said to be administered. For the present purpose I have not thought it necessary to translate the reports *in extenso,* but I have extracted from them all such portions as appear to me to bear more directly upon the point under discussion; and I would propose to deposit with the Society the accompanying accurate copies of all the reports and other documents from which these extracts are made, in order that persons interested in the inquiry may have opportunity of referring to the original papers at full length.

All the letters received from the medical men in Styria agree in acknowledging the general prevalence of a belief that certain persons are in the habit of continually taking arsenic in quantities usually supposed to produce death. Many of the reporting medical men have not had any experience of the practice; others describe certain cases of arsenic-eating which have not come under their personal

* *Sitzungs Berichte der Academie d. Wissenschaften,* 1860. Band xli. p. 573.

notice, but which they have been told of by trustworthy people whose names are given; whilst others, again, report upon cases which they themselves have observed. It is noteworthy that no one of the seventeen medical men denies, or attempts to disprove, the truth of the generally expressed opinion concerning the arsenic-eating.

The first question which I shall endeavour to answer will be — Is or is not arsenious acid, or arsenic in any other form, well known and widely distributed amongst the people of Styria?

Through the kindness of Professor Gottlieb of Grätz I received a quantity (about six grammes) of a white substance which the following examination showed to be pure opaque amorphous arsenious acid. The substance being subjected to a careful qualitative analysis gave all the well-known characteristic reactions of arsenious acid; the presence of no other body could be discovered.

The specific gravity of the substance at 15° C. was found to be 3·8059. (Opaque arsenious acid has, according to Taylor, the specific gravity 3·798).

Quantitative Analysis. — 0·6495 *grm.* of the substance was dissolved in nitric acid, the excess of acid expelled by heating on the water bath, the liquid then neutralised by ammonia and a solution of sulphate of magnesium, added with which so much chloride of ammonium had been mixed that ammonia produced no precipitate of magnesia. The precipitate thus formed was allowed to stand for twelve hours, then collected on a weighed filter, completely washed with cold dilute ammonia, and after drying at 100° C. for twenty-four hours it was weighed, and re-dried until no further diminution in weight occurred. 1·2465 *grms.* of the arseniate of magnesium and ammonium was obtained, corresponding to 0·6492 *grms.* of arsenious acid. The substance, therefore, contained 99·97 per cent. of $As\,O_3$, or is perfectly pure arsenious acid.

This arsenious acid, which was enclosed in a dirty blue paper bag, was brought by a Styrian peasant woman to the district judge of Knittelfeld in Styria; the woman stated that she had taken the parcel from her farm labourer, whom she had caught in the act of secretly eating the arsenic. The following certificate was forwarded to me with the parcel containing the arsenious acid:

(*Translation.*) — "This Hidrach* was delivered to me by a peasant woman from Mittenlobing whose name I am unacquainted with. She saw her farm-labourer secretly eat it, and gave it to me in order that the bad habit might be stopped.

HEUFENSTEIN, *Bezirksvorsteher.*

Knittelfeld, 26*th* April, 1860."

This shows that the substance called "Hidrach" by the Styrian peasants is arsenious acid, and not, as suggested by Dr. Taylor, oxide of zinc. The following extracts from some of the reports of the medical men before spoken of prove that this Hidrach is well known and widely diffused in Styria; they show also the sources whence the substance is obtained.

Dr. Oberhammer writes from Nöflach: "Arsenic is a substance well known to and in great request with the people of this district. It is chiefly and pretty generally used to be given to horses, &c." He also says: "I myself am unacquainted with any case of arsenic-eating. The clergy would be able to give the most information upon the subject. The clergyman Von der Graden showed me two pieces of arsenious acid which he had taken from a parishioner at the confessional."

Dr. Burghardt of Kindberg says: "In lower and central Styria there are a large number of glass works in which

* "Hidrach" is the corruption of "Hütten-rauch," by which name arsenious acid is generally known in mining districts in Germany.

arsenious acid is much used under the name of 'Hidrach,' or 'Hüttenrauch.' I know that arsenic-eating is most common in those districts, because the arsenic can there most easily be obtained."

Dr. Forcher writes from Grätz: "As long as the customs-frontier to Hungary existed, Hidrach was brought over as a much prized article by smugglers and others into the more remote valleys. The labourers in glass works obtained the arsenic from their master's stores; the arsenic works in Salzburg serve as a source of this substance for the neighbouring Alpine districts; in Orblau the red sulphide of arsenic, realgar, is obtained from roasting arsenical copper ores; and the arsenic works in the Reichenau-thal connected with the Cobalt works at Schlögelmühl is another source whence arsenic could be obtained in quantity."

Dr. Schäfer of Grätz attributes the large number of cases of poisoning which occur in Styria — for during the period of two years which he has acted as government Toxicologist in that district no less than thirteen cases of arsenical poisoning have come under his notice, — to the wide distribution which this poison enjoys in that country.

Dr. Holler of Hartberg mentions the following purposes for which arsenic is employed in Styria. (1.) It is eaten by men. (2.) It is used in the preparation of Styrian cheese. (3.) It is used by pregnant women to procure abortion. (4.) It is given to horses, cattle, and pigs. (5.) It is used as a poison for rats and mice. (6.) It is used for soaking wheat. (7.) The charcoal and lime-burners, as well as tile makers and potters, throw arsenious acid upon their fires.

Mr. Heisch in the paper published in the *Pharmaceutical Journal* gives the following interesting extract from a report by Professor Schallgruber of Grätz in the *Medicinischer*

Jahrbuch d. Œstr. Staates, 1822. The professor says: "The reason of the frequency of these sad cases (of poisoning) appears to me to be the familiarity with arsenic which exists in our country, particularly in the higher parts. There is hardly a district in upper Styria where you do not find arsenic at least in one house under the name of 'Hidrach.' They use it for the complaints of the domestic animals, to kill vermin, and as a stomachic to excite an appetite. I saw one peasant show another on the point of a knife how much arsenic he took daily, without which, he said, he could not live; the quantity I should estimate at two grains."

The sale of arsenious acid is strictly forbidden by law in Styria, and according to Dr. Holler two applications for permission to buy this substance were refused by the authorities. Hence it is of course somewhat difficult to get accurate accounts as to where the arsenic was obtained. The difficulty of procuring exact information respecting the arsenic-eating is also great, owing to the fact that the habit is carried on in secret and is generally disapproved of.

I shall now proceed to examine the question as to whether arsenic is or is not regularly taken by persons in Styria in quantities usually supposed sufficient to produce death.

I. *Cases coming under the personal observation of medical men.*

The most narrowly examined and therefore the most interesting and valuable record of a case of arsenic eating, is that described by Dr. Schäfer in the paper above referred to. The case fell under the personal examination of Dr. Knappe of Oberzehring. In presence of this medical man, a wood-cutter thirty years of age, who stated that he had taken arsenic for twelve years, and had always been in good health, did, on the 22*nd* of February 1860, eat a

piece of arsenious acid which weighed 4½ grains, crushing it between his teeth, and swallowing it. On the 23*rd* February he in like manner eat a piece of arsenious acid which weighed 5½ grains. During this period he consumed his food with his usual appetite, drank much spirituous liquors, and on the 24*th* went away in his ordinary state of health. He informed Dr. Knappe that he was in the habit of taking the above quantity of arsenic three or four times during the week. The urine which this man passed on the 21*st* instant, on which day he said that he likewise took a dose of arsenic, as well as that passed on the 22*nd*, was chemically examined by Dr. Schäfer, and in both portions the presence of arsenic was incontestibly proved. The details of the methods of analysis employed showed that the examination was made with due care, and that every reliance may be placed in the results obtained.

The following cases came under the personal observation of the reporting medical men; and although no chemical examination of the substance was made, and the weight of arsenic eaten was not determined, yet they appear of great interest, as helping to throw light upon the question.

Abstract of statements made by the reporting medical men.
(*Copies of the original reports in the possession of the Society.*)

Dr. Holler of Hartberg makes the following remarks :—
1. "I once made an expedition, in company with several friends, to the Alps in the Ens Valley. We were accompanied by two guides, and remained in a hut overnight. In the morning, on going to the spring to wash, I saw one of the guides carefully shake a white powder from a paper into his hand, and lick it off with his tongue. I asked him what the powder was; he answered that it was arsenious acid. On questioning him as to the reason of his taking

arsenic, he stated that when he was forty-five years old he had been overseer of a chamois-hunt, and that in this capacity he had so over-exerted himself that he became very ill, and by the advice of an old hunter began to take arsenic. His health then improved; he was able to return to his duties; and he had continued to take regular doses of arsenious acid for ten years up to the date of my communication with him. I have every reason to believe his statement, as he appeared to be a respectable, truthful person, and only gave me this information upon my repeated cross-examination.

2. Arsenic is used in Styria to flavour cheese, for the purpose, it is said, of giving it stomachic properties. I was once eye-witness of the preparation of such cheese. I happened to be in the Ensthal, in a hut where the dairy-woman was making cheese. A pot of curds was standing ready to be put into the wooden cheese moulds, when the woman took out from a small box as much powdered white arsenic as covered the end of a table knife, and shook it into an earthen vessel, in which about a pint of water was boiling. She stirred the water with a stick, and after it had boiled for ten minutes she poured the liquid in a thin stream into the curds, so that it was equally distributed throughout the mass of about eight quarts. She then kneaded the curds about for a long time, and afterwards filled the wooden moulds. Upon my asking her what the powder was, she said it was "Hüttenrauch" (arsenious acid), and that the Styrian cheese must be thus prepared in order that no accident may occur. She pointed out to me an inn in the valley where I could buy such cheese. There I went, and after much opposition on the part of the maid-servant, who was afraid that I should poison myself, obtained some of the cheese, but eat only such a quantity as the maid told me would be harmless, a piece the size of two walnuts. I eat some bread and drank

some beer to the cheese, and experienced nothing but a slight burning in the throat as from food containing much spices, and afterwards a pleasant warmth in the stomach, and good appetite. I ate such cheese several times again without any evil effects manifesting themselves. Once, however, in the neighbourhood of Murnau, I ate some Styrian cheese, which on mastication appeared to contain sand; I left the remainder untouched, and although I had eaten a piece only as large as a nut, I soon became very unwell, vomited several times, and was seized with colic. This cheese no doubt contained arsenious acid in the form of powder."

Mr. Stern, surgeon, of Kundorf, reports as follows:—"About two years ago I was called in to see a strong field-labourer thirty-four years of age, who it was said had been suffering for twenty-four hours from severe colic. On examination I found that it was a case of violent gastro-enteritis, and as the patient could give no reason for his sudden attack, and all symptoms pointed to poisoning by arsenic, I asked him if he had not eaten "Hüttenrauch," upon which he answered exactly as follows: About seven years since he had suffered from indigestion and general debility, and being always unwell he had followed the advice of an old man, and had begun to eat arsenic. After this he had gradually improved in health, and had since then regularly taken arsenic, and had been perfectly well until yesterday, when he took rather a larger dose than usual, and was seized with violent pains, which he thought were colic. Upon administration of the usual remedies, especially the hydrated peroxide of iron, the patient recovered in a short time."

Dr. Kropsch of Leoben relates that he was once called in to prescribe for a peasant who had been induced to eat arsenic to improve his health. Symptoms of acute poisoning set in, and the patient acknowledged that he had

eaten arsenic. Dr. Kropsch relates another case which came under his own observation of a felon upon whose person two pieces of arsenious acid, about two grains in weight, were found concealed. The prisoner begged hard that he might be allowed to keep these the last portions he possessed, and at last managed to swallow both pieces. He gave reasons for having contracted the habit of arsenic-eating, in which he had indulged for a length of time, and said that the quantity which he usually took at once was about two grains.

II. *Cases related to the medical men by trustworthy persons.*

The cases of arsenic-eating related by trustworthy persons to the medical men are of course by far the most numerous. These cases, though not so decisive as those coming under the personal observation of the reporters, have a certain value as expressing the opinions and experience of a large number of persons whose truthfulness there is no reason to doubt.

Dr. Holler, in his report, adds a list of persons in his neighbourhood who have given him information concerning the practice of arsenic-eating. He says: "I have had many opportunities during the twenty-four years of my medical practice to collect observations upon this subject. To my own observations I have added those of other trustworthy persons. As we have not here to support a theory but to prove a fact, which at first sight appears incredible, viz., that persons are able to eat arsenic for many years and yet remain strong and healthy, and that animals to whom this substance is given become fat and strong, I name the persons from whom I have the information in order that all doubt as to the truth of the statements may be cleared away." Here follow the names and addresses of fifteen persons. Dr. Holler then continues: "I and

the above-mentioned persons guarantee that together we are acquainted with forty arsenic-eaters. They are all men from thirty to seventy years of age, by occupation charcoal-burners, stable-men, field-labourers, innkeepers and traders."

The following names of and particulars concerning arsenic-eaters are collected by Dr. Forcher of Grätz. He does not say that he has personal knowledge of any of the cases; he gives them as facts, but only once or twice mentions his authority for the case.

1. Johann Wolch *vulgo* Gföller in Mitterbach, took arsenious acid daily, beginning with pieces of the size of a pin's head, and gradually increasing up to the size of a grain of oats. Died of the effects of the poison. An inquiry as to cause of death was held. Particulars concerning this will probably be found in the district offices of Knittelfeld or Judenburg.
2. Peter Flockmayer of Mittenfeld, ate arsenic, which he also gave to his horses. Died of typhus.
3. Josef Hiebler of the 1*st* Grenadier Company of the 27*th* Infantry Regiment, ate arsenious acid of the size of a grain of wheat.
4. Tuschler in Knittelfeld, took arsenic, and raising the dose, was seized with symptoms of poisoning. He recovered.
5. Maria Windisch, a cowherd in the district of Obdach, was in the habit of eating arsenic, which she continued to her old age. The quantity taken at once was the size of a grain of wheat. She died in Obdach in 1858.
6. Kutscherwirth in Brück, takes arsenious acid, but conceals both the frequency and quantity of the dose.
7. Heinreich Kittner, Captain in the Engineers, sta-

tioned in 1859 at Carlsburg, took a piece of arsenious acid as large as a grain of sand every evening.

8. Herr Wernberger, formerly residing in Tuffer, now probably living at Zilli, took arsenious acid for years. He used to scrape the quantity he required from a larger piece. He was of advanced age, and had always enjoyed good health.
9. George Münzer, now dead, worked in the iron mines in See-thal near Obdach; he took every week a piece of "Hidrach" of the size of a grain of wheat. This information is derived from his fellow miners.
10. Mathias Sittler, always in good health, died in Obdach at the age of seventy-one; believed that he owed his good health to the habit of taking arsenic several times a week on bread. His son-in-law, Ottenig, blacksmith in Obdach, guarantees these facts.
11. Caspar Graf, now in service at the forge in Obdach formerly ate a small portion of arsenious acid every day. During this period he never was unwell.

The foregoing are the most important facts regarding the alleged practice of arsenic-eating in Styria which I have been able to collect. A great deal of other interesting matter will be found in the full reports presented to the Society. A most important case of administration of no less than 555 grains of arsenious acid to a horse in twenty-three days, without any evil effects being produced, is related by Dr. Schäfer.

I have, however, confined myself for the present to the discussion of the two points above mentioned, and I believe that the evidence brought forward is quite sufficient to justify the conclusions:

I. That arsenious acid is well known to and widely distributed amongst the peasants of Styria.

II. That arsenious acid is taken regularly into the system, by certain persons in Styria, in quantities usually supposed sufficient to produce immediate death.

XV. — *On the Production and Prevention of Malaria.* *By* Dr. R. Angus Smith, F.R.S.

Read February 19th, 1861.

1. *On the Production of Malaria.*

By malaria I mean simply bad air rising from land or water not contaminated with impure substances from the habitations of man. In this short paper I by no means propose to myself to enter upon the whole question of the production or prevention of malaria. The subject would require not only more time than can be given to it on this occasion, but much more information than I possess. It has been my intention to examine the matter thoroughly, but I have not had the necessary opportunities, and I give here only some ideas towards an elucidation of the subject, founded on a few simple observations. It has been shown by numberless writers that malaria is a very frequent evil; that large portions of Europe are more or less subject to it, and considerable districts rendered by it quite uninhabitable. But besides those dreaded evils, such as almost instantaneous illness or slow and lingering death, Dr. Macculloch has shown that large portions of very healthy countries, such as England itself, are subject to emanations which diminish their healthiness to an extent which careful observation can detect, although the sufferers themselves are unable to account for the loss of strength, or look on it only as one of the inevitable evils incident to

humanity. Macculloch says: "I trust to prove in this essay, that the causes of malaria exist under numerous circumstances not at all suspected in our country, and in thousands, tens of thousands of places, even at our very doors" (p. 11, ed. 1827). Again: "Malaria produces in itself a far wider mass of human misery than any other cause of disease; as, for the world at large, it is also the cause of far more than half the mortality of mankind" (p. 31).

In the examination of different districts for organic matter in the air, I have invariably found that low grounds well cultivated have shown more than high grounds or hills, and more than the open sea. I cannot, therefore, doubt that the remarks of Dr. Macculloch are correct as to the very general occurrence of unwholesome influences caused by the state of the ground. I am inclined to go further, and to say that even in places where we cannot find that health is in any way injured, emanations arise. But it is not sufficient that health should be injured, or at least the average age reduced below the standard of the country, in order that a place may be called insalubrious. It may be that the impulse given to life may be diminished in some districts by substances rising from the ground, whilst in other districts an entire or greater freedom from all noxious exhalations may allow the supply of the purest air, with all its highest quickening effects.

The cause of malaria has been with great certainty traced to the soil. I shall not revert in this place to the arguments and observations which have led to this conclusion. It is true that some places are exposed to emanations of gases from subterranean reservoirs, but gases such as can be prepared by the chemist, combinations of inorganic bodies, are not known to be able to produce the agues and peculiar fevers which we connect with the true malarious district. Atmospheric phenomena, the condition

of electricity for example, affect the health, and so does the amount of water, of cloud and of light; but all these causes seem to be different from that insidious poison arising from those organised bodies found neither deep in the earth nor high in the air, but only on the surface of the ground.

It is now many years since I first began to study these subjects; and one or two of the facts which I allude to, and which I consider important, were published fourteen years ago; but their significance has not till lately been sufficiently clear. I observed that the water of soils is sometimes acid and sometimes alkaline; that on the surface generally of our soils it is acid to a small extent, whilst below, when the inorganic salts overpower it, it becomes alkaline. We may say as a rule that all inorganic soils give off their drainage water in an alkaline state. The tendency of plants is to render the soil acid, and if they render it very acid the soil is said to be sour. Drainage will draw off the excess of vegetable juices, and lime will also remove them, as well as other free alkalies. In hot-houses and where rich manures are used, the soil is not always acid, but frequently very alkaline. I have not examined a great many, but where forcing is resorted to with strong manures the result must be and is an alkaline condition. This condition depends on temperature. Having observed some very alkaline soils, I went a few days afterwards for a specimen of water flowing from the surface, without allowing it to pass deep into the strata which would have filtered out its organic matter, and given it alkaline earths. Meantime the weather had changed, and the alkaline water had disappeared, having given place to one that was acid. This was in a peaty district.

If the soil of our fields were not kept acid, but were kept alkaline and warm, there would arise from it a

greater amount of exhalations. "If the soil is very alkaline and moist, the conversion of the organic matter into ammoniacal compounds is very rapid. I put some soil not very rich in organic matter into this condition by the assistance of a little ammonia, so as to make it alkaline, and the consequence was the rapid occurrence of a very intense putrefactive decomposition, not in any way differing, as far as could be perceived, from that of ordinary putrefaction of animal and vegetable matter. These nauseous and unwholesome odours are therefore possible from the ordinary soil of our fields; but any occurrence such as this on a large scale would be disastrous, and the ground is protected from it by an almost constant acidity, which sometimes increases so as to be injurious, forming what is called sour land. This very acid state generally occurs in wet land, where it is probable that alkalinity would be most injurious, but the soil may be found alkaline in a well-manured garden, and where the ground is dry, without apparent injury." — Author's *Report on Air and Water of Towns*, *British Association* 1851, p. 69.

In this experiment I produced in fact *artificial malaria*. The conditions were exactly such as the best observers have in most cases described, with the addition of the alkaline state, which will probably be found to play a very important part, but which as far as I know has not been attended to. Not that I say that it is impossible to have malaria without ammonia, but ammonia is the wing on which some of the products of decomposition seem to delight to fly. This experiment, which brings the usual condition of warmth and moisture, shows that there may be many modes by which the organic matter in land may pass from the soil besides the transformation into plants, and besides simple oxidation. The decomposition spoken of shows that from soil numerous products may arise. If we allow that putrefaction begins we know no end to the

number of substances produced. These products will vary according to the nature of the substances in the soil, according to the season when one or other substance predominates, and when one or other substance acts as a ferment.

It is not necessary to go further than this experiment in order to show that there may arise from soil under certain conditions substances producing the most desolating diseases. I suppose this artificial state to be the extreme, such as probably has never happened in an extensive district. All other conditions of insalubrity are probably stages of this. It may be said that it was well known that soil retained organic matter, and that organic matter could putrefy, but this state of actual decomposition had never before, as far as I know, been observed. By the sense of smell we have found reason to believe that volatile products arise; disease has also led to this indication, and minute quantities have even by careful experiment been observed; but in the experiment I speak of, the amount rising was gross and unquestionable. I will scarcely venture to speak of the quality of these products. To me they strongly resembled those of putrid flesh or blood, and in that case their composition would probably ally itself to that of protein; indeed I find that after removing the purely gaseous bodies arising from such decomposition there is left a substance whose carbon and nitrogen bear a relation almost identical with that in protein. But the variety of substances found will probably prove to be extremely great. We can imagine some washed down by the rain, and some removed by the plant. In the same way some classes of malarious diseases may be removed by rain, some caused by more and some by less moisture and heat, and others better removed by the agency of plants and the processes of agriculture.

The soil is an extremely complex machine; the balance

of forces in it has not been studied. The surface is acid, the lower part is alkaline; it is a filter, with a power of retention and selection. The flow of air and water is from the surface downwards. The surface therefore becomes first oxidised, and is made acid. This acidity is itself a preventative of great putrefaction, and this oxidation is a destruction of those substances which may have resulted from the rotting masses. When land is well drained these processes will go on with greater rapidity; it is as if the lungs were enlarged. But notwithstanding this oxidation and prevention of decomposition we must not forget that it is useful to counteract another agency of great importance, that by which the plant rots. This rotting is accompanied by grubs, insects, animalcules, and vapours and gases; it closely resembles, and no doubt in part is a suppressed putrefaction. The soil has the power of retaining the products to a great extent and until it has destroyed them, when they are not excessive.

If the passage of air through a soil is not sufficient the products of decomposition are not destroyed. If the passage of water through a soil be not sufficient the soluble products are not removed into the soil, but left, and assisted to putrefy.

It is certainly an object of great interest to be able to know by the examination of a soil if it could or would produce malaria, or if it were really in the act of producing it. By the view taken most soils, indeed all containing putrescent organic matter, will be subject to malaria when the oxidising influence of the air and the balance of vegetable and chemical life are disturbed. The soils most productive of malaria are low grounds, and moist ones with abundant vegetation. At the same time there are others not described as such, and not very thoroughly investigated. If we examine the surface water flowing from such grounds in this country we shall find it to contain animal

or animalcular life in immense variety, and we shall also find that if we allow it to stand vegetable life will show itself in great abundance. The hilly districts of our own island do not contain water of this kind. I was extremely surprised, on taking my microscope to the north-west of Scotland, to find that the ditches were so meagre of living creatures. A green pool was scarce, and when found it was small, whilst on examination it was vivified by very few living forms. The whole hills in fact were either void of matter capable of putrefaction, or they were so rapidly washed that the putrefying matter was passed at once into the soil. The moisture, too, produces no corruption; it is water in rapid motion filled with air, and rapidly traversing the soil, oxidising all the small portions of putrid matter that may be forming in the soil, and fixing the products in the earth. The animal life is repressed, *i.e.* the life of such living things as arise from decomposed plants or animals. But why should this be? Let us push the argument further. If grass grows in such places there must be food for plants, and elements for the production of infusoria. It is true that grass grows, but not in great vigour, and as the rain passes very rapidly through the soil there must be very little matter kept soluble at one time. The plants must be fed by small although by very frequent meals. Besides, the soil is not deep; the power of retaining soluble matter is therefore small; the stock of food laid up over the whole district, in other words the material from which such animal life must spring, is extremely scanty. Now I am not prepared to say that all unhealthy districts actually contain a large amount of matter either putrefying or forming into animalcular life, but it certainly is the characteristic of what seems to me to be the soil most blamed; there may be some, as already stated, where other causes arise. This state of soil is that of all our lower lands; the ditch water swarms with life;

muddy wet lands present it in every drop of water; whilst in our high hills, purified by barrenness and disinfecting peat, we have hunger and health. I do not say that there is a necessary connection between malaria and animalcules, but as there is a connection between them and the state of the air in our own county, it is probable that the inquiry may be extended profitably to malarious districts. Infusoria and even larger animals indicate the presence of matter which may putrefy, but it is even probable that they may prevent its injurious action on the atmosphere.

The microscope then may very soon be brought forward as an instrument for ascertaining the sanitary condition of a country. It is extremely probable that the quality of the decompositions will be known by the microscope to a large extent, and the nature of the disease and its extent be indicated. It was not my intention to have brought forward these views until I had examined in detail this part of the subject, but I fear I shall not soon have an opportunity of doing so, although I have more materials ready than I can find room for in this paper, which is chiefly a practical one. I may readily be misunderstood. I do not say that where most animalcules are there is most disease. Water may exist too putrid for them, and some states of decomposition destroy them.

It may be said that it was well known that places containing less organic matter were less liable to disease, and that the sea coast and barren hills have long on that account been frequented; but to me at least the reasoning generally and the mode of observing is new, and I feel as if new eyes were given us for observing the soil more minutely. To some extent we can at once tell the quality of waters by the use of the microscope, and have even a clue to the gases and vapours rising. If these waters are taken from the surface of land we have a clue at once to the condition of the land.

2. *Prevention of Malaria.*

If we can imitate the production of malaria, can we not also imitate the mode by which it is destroyed or prevented? When the products of decomposition are formed in a soil they are removed by natural processes. Mere mixture with the soil will remove or render decomposing matter innocent. The soil will act as a porous body. But the soil may be overburdened. It may be shallow, and its machinery may be of small force; or it may be inefficient from the excess of organic matter over the amount of air passed through it. The first act of disinfection is the action of the soil as a porous body. The next seems to be the act of oxidation by which the soil at the surface is rendered acid. By these means decompositions are confined within the soil itself.

We may consider the atmosphere to be in a constant struggle with the vegetable matter of the soil. Substances containing nitrogen are constantly tending to give out ammonia, the ground is constantly tending to convert this into nitric acid, whilst it converts other portions into carbonic and organic acids. By manuring we assist the tendency to become alkaline, taking the side of vegetation, and we resist the oxidising tendency of the air. A large portion of our manuring is simply the addition of alkalies, a struggle against the acidifying influence of the air. No wonder it requires so much lime for the fertilizing of acid peat lands. From lands of this kind it is probable that no miasma arises.

Moisture lying on rich lands becomes filled with animal and vegetable life, a sure sign of rapid decomposition. Allow this moisture to pass through the soil and this animal life disappears. This is an act of purification by drainage. It may not at all times be possible to obtain the requisite amount of drainage, and there are even cases where the land produces malaria, without, according to the

greatest authorities, showing need of drainage. We can readily imagine one condition which will produce this result, viz. excessive dew, which will evaporate and carry a large amount of organic matter;* others may depend on the plants and animals present. The oxidation in the soil may also be aided by the artificial opening of its structure, but this may not always be convenient, and the demand for it may be too laborious. Are we not able to imitate some of the other methods taken by nature to prevent infection?

If we add any acid to the putrid soil of which I spoke, that peculiar mode of decomposition ceases; if we add any other disinfectants or antiseptics we put a stop to decomposition. If we add antiseptics to the water over soil containing a great variety of animalcules, and giving every evidence of decomposition, we find these instantly dying; the same, of course, results with grubs, larvæ, &c. Animal life is arrested, and chemical action is staid or impeded according to the amount used; but a very large amount must be taken in order to show an injurious effect on vegetation. In this experiment we do in reality prevent malaria; we arrest the decomposition of substances in the soil. We cannot call this a theory or a speculation; it may be considered simply as a fact. The use of antiseptics will arrest all animal and vegetable decomposition. and where there is neither of these malaria will not arise. The chemical action which goes on in a vessel containing a few cubic inches will not differ if extended over a surface of miles. I look on the results, therefore, as certain, viz: that by the use of disinfectants malaria will be destroyed.

The idea of the disinfection of whole districts rose out of a proposal made long ago to disinfect whole cities by beginning at the root of the evil, the sewers. That was

* Since writing this an Italian friend informs me that this opinion is prevalent amongst the scientific men of his country.

intended for this country, the land of great cities. This plan for the disinfection of districts is intended chiefly to apply to other countries. I may, however, be allowed to give one illustration from this country. Mr. McDougall has used sewage over one hundred acres of land; sheep and cattle have fed on it without any case of disease. The growth of vegetation was great, the moisture was constant; there was also the constant presence of decomposed matter, but there was no disease. There was, on the contrary, a large production of healthy vegetable and animal life. This experiment suggests many questions, but I do not intend to enter on any except so far as to show it as a proof that putrefactive decomposition may be arrested without any fear of destroying the life of plants, and that rich water meadows may be used for feeding cattle without fear of rot, even if treated with sewage. The disinfectant used was crude carbolic acid with lime water.* The arrest of decomposition was thus caused by a process which at the same time diminished the acidity. I think the amount required will be small enough to render the process possible over large districts. At the same time also I expect that the idea will lead to other and perhaps even to cheaper modes of producing a like result. As it is to be hoped that an opportunity of using it will occur, it is well not to attempt to speak of details.

I may add also at the same time that the extinction of insects obnoxious to a country, — gnats, mosquitoes, and those dreadful enemies of cattle the zimb and the tsetse, — will probably follow the destruction of the obnoxious decompositions in the soil. Nor do I suppose that only one disinfectant will do the work, although the one I

* The value of carbolic acid has elsewhere been shewn by Mr. Mc Dougall and myself; the preparation here used was that patented by Mr. McDougall in 1859. He will be able soon to make public many other experiments relating to rot and the destruction of insects.

have mentioned is, as far as I know, the most powerful, and can be made in all countries and climates where coal lies or where trees grow.

It is desired to make Rome the capital of Italy. From all I hear it is badly fitted for such from its sanitary condition. By the method proposed I believe objection on the plea of health would be removed, and it would rapidly be made fit for habitation.

I wish in this paper chiefly to show that decomposition, to a most pernicious extent, is possible in soils; that it is not a mere opinion, but a fact readily demonstrated. Next, to show that decomposition may be arrested artificially to the preservation of health without the destruction of vegetation; and that in these facts we have not only a surer basis in our reasonings on the origin of malaria, but an almost certain process for its ultimate and total destruction.*

* Since writing the above I have formed artificial malaria in soil by the use of water only, an exactly analogous case to that of marshes. I call it artificial malaria, although, of course, I have not produced fever or ague with it. Similar soil, when disinfected, evidently resists animal and chemical decomposition longer, yielding at once to the wants of vegetation. It is said that cultivation will cure malaria, but malarious ground is dangerous to touch; our engineers are dying rapidly whilst making the railways in India. The soil and the jungle could both readily be treated before turning them up, or as soon as turned up. I may add that the road to and from India might be made more wholesome by the destruction of the corruption in the bilge water of ships; nothing can be easier, and yet men die on account of this corruption, and people have inquests and commissions upon them instead of commanding its cure.

XVI. — *On the Kaloscope.* *By* Mr. W. H. Heys.

Communicated by Mr. George Mosley.

Read February 5th, 1861.

One of the objections frequently advanced against the use of the microscope is that it injures the eyes. All observers are aware that they are constantly using a light which is at least unpleasant from its brightness, if not positively hurtful; and attempts have long been made so to modify the light that the observer may work on through a long evening without pain or fatigue.

It is hoped that the "kaloscope" will meet the wants of all those who use the microscope frequently, enabling them to pursue their investigations with the long-desired comfort, and induce many persons to commence microscopic studies who hitherto have been deterred through fear that injury to the sight would be inevitable.

The kaloscope consists of eight discs of coloured glass, the discs having a uniform diameter of 2½ inches. They are mounted upon a stand of 12 inches in height, similar to that used for a bull's eye condenser, but larger, and are arranged in two sets of four glasses each, an upper set and a lower set, each set being attached to an arm which can be moved about in any direction; and as every separate disc has its own independent motion, it will be obvious that any angle of light may be obtained. The upper set of glasses is fitted to a tube, which slides upon the upper portion of the pillar of the instrument so as to allow of

their being moved about without disturbing the position of any of the glasses belonging to the lower set which may happen to be in use at the same time.

The kaloscope can be used for a very considerable variety of objects; and a great number, which do not polarize, are made to disclose all the beauties of polarized objects. For instance, suppose the anthers of the mallow, with their pollen, to be the object under view; we place the slide upon the stage, arrange it as a transparent object, *and also throw condensed light upon the upper surface;* then placing the kaloscope in position, or so as to allow of a red glass belonging to the lower set being interposed between the lamp and the mirror, the ground upon which the object is seen is coloured crimson. By next adjusting a green disc belonging to the upper set (green being the complementary colour of red) so that its tint shall be thrown through the bull's eye, a beautiful green hue is cast upon the object, illuminating it in a perfectly novel and most valuable manner, the anthers and pollen appearing intensely green, while the ground beneath them is crimson. These colours may be changed for others at the pleasure of the observer.

It will soon be discovered by any one using the kaloscope that nearly all objects viewed by it appear so much in relief that we might suppose them to be viewed through a stereoscope. For observing anthers, jointed hairs, oil glands and vegetable sections in general, but especially sections of wood, this instrument will be found, accordingly, of singular use. Some objects, moreover, which before were attractive only to the scientific botanist, are transformed into pictures of such beauty that they cannot fail to rank in future with the most popular and esteemed microscopic preparations. The calyx of the moss-rose is a good example of both these results. Under the ordinary modes of illumination it is a mere entanglement of coarse

fibres, with dark beads sprinkled here and there. Viewed by the kaloscope it is instantly transformed into a most lovely stereoscopic vegetable branch, while the glands at the extremities of the twigs glitter like diamonds.

Sections of wood, spines of echini, and similar objects, may be viewed in two entirely distinct ways. By the plan already described for the mallow they will be found as beautiful as when examined with the polariscope. By the other arrangement, now to be described, the details are brought out in a wonderful manner, and better than by any other mode of illumination. The slide being placed upon the stage, the most oblique light possible must be thrown up through it from the mirror. The kaloscope is then placed between the lamp and the mirror in such a position that the object is *fringed* by the coloured light, while the ground remains *intensely black*. This method of illumination may be used with great advantage in examining the hairs upon the edges of leaves and petals, and those also which often spring from the filaments of stamens.

The following plants supply remarkably beautiful objects of the class in question:

Tradescantia Virginica (stamens).
Lamium maculatum (stamens).
Achimenes (petals).
Hypericum pulchrum.
Lysimachia vulgaris.
Stellaria media.
Salix (male flowers).
Salvia splendens (hairs on corolla).
Asperula odorata (spines on fruit).
Narthecium Ossifragum (stamens).

It may be added that it is in many cases sufficient to use a glass *of the lower set alone*.

The pollen of the Malvaceæ, for example, may be shown with a blue ground from the kaloscope, while the surface

is illuminated in the ordinary way with the bull's eye. It then forms a highly attractive and pleasing object.

It is thought that the efficacy of this instrument in connexion with such a variety of purposes cannot fail to render it of much value to the scientific observer. It most certainly enables him to detect details of structure which are inconspicuous without its aid. By the exquisite beauty with which it paints many kinds of objects it will also allure the beginner to continue his researches at a time when probably he might abandon the pursuit as tedious.

To all who have refrained from using the microscope through fear of hurting their eyes, it can further be recommended as calculated to remove all anxiety; for when the blue glass of the lower set is used, even if no other end be gained, the light is delightfully toned down, and in a way that can only be appreciated by those who have worked at the microscope for many hours at once.

XVII. — *On Meteorological Observations, and Observations of the Temperature of the Atlantic Ocean, made on runs from Liverpool to Gibraltar, and from Gibraltar to Liverpool, in September* 1860.

By THOS. HEELIS, F.R.A.S.

Read before the Physical and Mathematical Section, January 31st, 1861.

THE runs during which these observations were taken were made in large iron screw merchant steamers, the property of Messrs. Meadows and Bibby. The run out was made in the ship "Crimean" of (I think) 1006 tons register. The weather during the run was fine and moderate, with smooth water almost the whole way, the little swell that occurred being met with in the southerly part of the Bay of Biscay. The winds along the coast of Portugal were light at S. and S.S.W., with smooth water.

The passage home, on the contrary, was stormy and boisterous, the ship ("The Rhone" of 964 tons register) having left Gibraltar in a gale of wind, and having met with bad weather, with a heavy westerly and north-westerly swell, caused (I think) by the approach of the hurricane which ravaged the north of Scotland and the Baltic on the second of October and following days, of which more hereafter.

The observations of the air and surface water were taken with a thermometer supplied by Mr. Dancer, and the requisite corrections to reduce them to the readings of

the standard thermometer at Kew have been applied. Unluckily soon after entering the Bay of Biscay on the return voyage, and when the observations (which were undertaken chiefly for the purpose of ascertaining whether the system of currents known to exist in that Bay could be ascertained by the temperature of the water) were becoming most interesting, the thermometer was broken, and the return series is thus incomplete.

On the run out the gradual deepening of the water is the first thing which tells notably on the thermometer. Indeed the delicacy of the action of the instrument at depths of from 80 to 100 fathoms and upwards is surprising. On referring to the table of observations it will be found that the thermometer, showing the temperature of the sea water, fell on the 11*th* September from 65·1 to 64, although the ship was standing to the southward at the time, and no land was in sight. On examining the chart it was found that the depth had decreased from 130 fathoms to 80. Hence the fall.

I cannot account for the two observations made at noon and at 9 p.m. on the 9*th* September being identical, especially as the temperature of the air was not the same in both cases, and the second observation was taken nearly ninety miles S.W. by S. from the first, except by supposing the ship to have passed twice through a cold current at two points of its course.

The influence of the shoaling of the water is clearly shown in the first observation on the 12*th* September, which was taken inside the Burlings, and in which the temperature of the water had fallen from 65·1, as shown by the last previous observation, to 60·2. The mixing of the waters of the Tagus with those of the ocean is clearly shown by the fall in temperature, the temperature off the mouth being on the run out 62·525.

This brings us directly to a difficulty raised by the

second series of observations, which were taken by the writer with the same, or if possible, greater care than those taken on the run out.

On the run home, in passing the mouth of the Tagus, the ship was further in the offing than in the run out. Hence it might reasonably have been expected, as only twelve days had elapsed since the former observation, that the temperature of the water would have been higher. It is, however, shown to be lower, the temperatures of the air being 61·11 to 65·35, and those of the water being 60·125 to 62·525. The differences of temperature in the two sets of observations are very striking, and show how impossible it is to arrive at any accurate determinations without long continued observations.

A few words as to the mode in which the observations were made may be desirable.

On the assumption that the vessel would not exceed a speed of ten knots an hour, which under steam and sail was considered good speed for the "Crimean," it was decided to take the observations as far as possible every three hours, which would give one to each half degree; but the observations were not continued during the whole night, as no practical experience in reading thermometers was possessed by the officers of the ship. The thermometer was generally hung to the belaying pins, and under the lee of the mainmast, for the purpose of obtaining the temperature of the air. For the water it was sometimes towed over the taffrail, and at other times immersed in a bucket of sea water drawn for the purpose. I found no difference in the readings obtained by these two methods. The barometer readings were taken from an aneroid which was carefully compared before and after the series with a standard barometer.

The following are the observations:

Date of Observation.	Temperature of Air corrected.	Temperature of Water corrected.	Barometer.	Longitude, Latitude, and Remarks.
1860, Sept. 8	61·6	57·2	30·270	53°7′ N. 4°47′ W.
	58·15	58·40	30·237	Off Smalls.
9	57	58·2	30·200	
	60·2	57·8	...	
	58·6	59·15	30·145	49°27′ N. 6°51′ W.
	59·85	59·90	30·100	
	60·2	60·1	...	
	60·1	59·15	30·037	
10	64·1	61·9	29·880	
	64·2	62·2	29·890	45°29′ N. 8°0′ W.
	65·1	62·25	29·940	
	63	63·1	...	
	62·6	63·1	30·000	
11	64	65	30·075	
	67·1	65·1	30·067	41°52′ N. 9°26′ W.
	67·1	64	30·065	
	64·1	65·35	30·095	
	62·85	65·1	30·100	
12	61·1	60.2	...	
	65·35	62·525	30·180	Inside Burlings.
	70·07	66·25	30·190	Off Tagus.
	69·4	64·5	...	38°12′ N. 9°24′ W.
	64·7	64·3	30·200	
	64·35	65·6	30.235	
13	66·2	68·15	...	
	68·1	69·1	30·200	
	67·1	69·1	30.155	5°49′ N. 6°9′ W.

The table of observations made on the passage home reads upwards from the bottom, to allow of the more easy comparison of the observations with the preceding ones. The following are the observations:

Date of Observation.	Temperature of Air corrected.	Temperature of Water corrected.	Barometer.	Longitude, Latitude, and Remarks.
	58·2	61·620	29·490	44°2′ N. 9°23′ W.
1860, Sept. 27	60·125	59·35	29·500	
	60·125	62·2	29·820	
	60·125	60·130	29·890	
	64·3	63·1	29·940	
	64·5	64·1	30·040	40°45′ N. 9°27′ W.
26	65·1	63·1	30·100	
	61·11	60·125	30·170	Off Tagus.
	63·1	61·35	30.140	
	62·4	64·1	30·130	
	60·2	63·1	30·140	37°32′ N. 9°4′ W.
25	60·125	62·1	30·150	
	61.325	67·1	30·020	
	65·1	67·3	30·020	
	64·1	...	30·000	
Noon 24	62·5	66·9	30·020	35°56′ N. 6°42′ W.

From the 27*th* September the barometer gradually rose, attaining on the morning on the 1*st* October at 9 a.m. a maximum height of 30·420 inches, from which point it fell under the influence of the hurricane which followed.

So early as the morning of Sunday the 30*th* September the hurricane, which ravaged the north of Scotland and the Baltic on the 2*nd* October and following days, was visible off Scilly as a low bank of grey cloud gradually rising up in the west and north-west. The sun went down behind this, producing an intense scarlet and very wild effects. On the 1*st* October a red sky was noted at sunrise, and the same cloud grew and grew until by the time the ship reached Holyhead it had covered the whole sky, and was flecked over with scud, or what sailors call "water casks." On the evening of this day there was also a wild and red sunset. The barometer had begun to give way as early as noon on the 1*st* October, and yet the shipping on the west of Scotland were, to some extent at least, caught by surprise.

We had experienced north-east winds all across the Bay, and along part of the coast. These were, I have no doubt, the drafts setting down to fill up part of the comparative vacuum caused by the progress of the storm.

XVIII.—*On Changes of Density which take place in Rolled Copper by Hammering and Annealing.* *By* Mr. Charles O'Neill.

Read March 5th, 1861.

A gentleman engaged in the application of copper to a certain purpose in the arts believed that it was possible to compress or condense the best commercial rolled copper so as to increase its density and improve it for the purposes he had in view, which I may state were in connexion with engraving or etching upon the metal. He devised or applied a powerful machine for the purpose of testing this idea, by means of which a great power could be brought to bear upon the copper by a succession of blows acting through a regulated space on the principle of the genou. The results were not satisfactory, and I was applied to in order to ascertain what was the actual increase of density obtained by a moderate amount of hammering. The first experiments I made did not show that there was any increase of density, but pointed in the opposite direction, intimating a loss of density. Nobody was inclined to accept this result, but further experiment confirmed it, and then a regular series of experiments was made to test the point, which resulted in a further confirmation of the earlier experiments. It was afterwards found that annealing the hammered pieces of copper increased their density, bringing them almost exactly to the same density they possessed before hammering. I proceed to give details of several of the experiments.

First Series.

I had ten pieces of copper cut from different parts of the same sheet, which was represented as being best rolled copper. The thickness was about $\frac{3}{16}$ inch, and the pieces weighed from 252 to 320 grains, each being rather more than an inch square. I cleaned them with dilute nitric acid and brickdust, so as to get a clean surface, and then took the density of each piece by a balance which would indicate clearly the 100*th* of a grain, and all the weighings were made to that fineness. I obtained as follows:

No. 1	8·884	No. 6	8·881
2	8·885	7	8·880
3	8·884	8	8·872
4	8·875	9	8·870
5	8·884	10	8·874
Mean density	8·879		

The pieces were then separately hammered in the genou, each receiving about fifty blows; the blows passed through about $\frac{1}{16}$ inch, and the pieces being at liberty to extend, they did extend perhaps one-half, but without any cracks or fissures. They were perceptibly warmed by the hammering, though the massive cast iron bed did not allow them to get very hot. After the hammering they were carefully cleaned, and their density again ascertained with the following results:

No. 1	8·850	No. 6	8·866
2	8·856	7	8·855
3	8·851	8	8·854
4	8·856	9	8·855
5	8·848	10	8·863
Mean density	8·855		
Loss on density	0·024		

The pieces were now annealed by putting them in red hot sand, and letting them cool slowly. They were then cleaned carefully and their density taken as follows:

No. 1	8·882	No. 6	8·886
2	8·885	7	8·884
3	8·883	8	8·885
4	8·882	9	8·885
5	8·883	10	8·885
Mean density	8·884		

Mean density of rolled copper	8·879
Mean density of hammered copper	8·855
Mean density of annealed copper	8·884

The densities of the annealed specimens agree very well, the greatest divergence from the mean being ·002, but in both the rolled and hammered pieces the error is much greater. But the plus and minus errors nearly balance each other in these cases, and do not, I think, depend upon the manipulation but upon actual differences in the copper. If it is so, it is interesting to note that the hammering and annealing have reduced the pieces to a nearly uniform density.

I got another piece of sheet copper, thicker than the former one, and said to be of a better quality; and in fact a very close examination of the surface showed that it was free from the minute perforations and crevices which could be perceived in the first sample experimented upon. I had ten pieces cut from the sheet, weighing each from 420 to 520 grains. Their densities were as follows:

No. 1	8·899	No. 6	8·897
2	8·900	7	8·901
3	8·901	8	8·897
4	8·898	9	8·899
5	8·897	10	8·898
Mean density	8·898		

Being hammered the same as before, the densities found were:

No. 1	8·879	No. 6	8·882
2	8·879	7	8·879
3	8·876	8	8·876
4	8·880	9	8·877
5	8·877	10	8·875
Mean density	8·878		

They were then made red hot, and allowed to cool slowly; carefully cleaned from adhering oxide, and the density of five of the pieces found as follows:

No. 1	8·890
2	8·906
5	8·899
7	8·892
9	8·893
Mean density	8·896

Mean density of rolled copper	8·898
Mean density when hammered	8·878
Mean density when annealed	8·896

The other five were lost or not weighed.

I made an experiment to try if this loss of density was progressive, and depended upon the amount of hammering. I took a piece of square bar of copper, 1·95 inches long and 0·5 inch square, and hammered it myself upon an anvil, taking its density in all eight times, each successive hammering being more severe than the previous one. I tabulate them as follows:

Sp. gr. of bar copper (weight 1097·35 grs.)	8·885
Hammered slightly	8·887
2nd Hammering, length 2·10 inch	8·884
3rd Hammering, length 2·20 inch	8·882
4th Hammering, length 2·50 inch	8·876
5th Hammering, length 3·00 inch	8·871
6th Hammering, length 3·50 inch	8·869
7th Hammering, length 4·50 inch	8·867
8th Hammering, length 5·50 inch	8·867

It lost five grains in weight during the successive hammerings, and was several times made very hot. The last hammering cracked the edges, and the recorded density may not be correct on account of the difficulty of taking it. The difference in density was 0·018.

In the two first series of experiments, by omitting the third decimal when under five and adding one to the second when above five, we get results perfectly alike in each determination of the second series upon which I place the most confidence;* and the effect of hammering and annealing may be stated as follows:

	2nd Series.	*1st Series.*
Rolled copper has a density of	8·90	8·88
When hammered	8·88	8·86
Hammered and annealed	8·90	8·88

The decrease of density, then, is about 1 in 450; that is, the copper has increased in bulk that much. The cubical expansion of copper by heat is about $\frac{1}{35000}$ for a

* But in fact the weighings justify the three places of decimals.

degree of Fahrenheit; an increase of heat of 78 degrees would expand the copper about $\frac{1}{450}$. This increase of heat I ascertained to be produced. If there be not a connexion between the heat produced and the decrease of density I can conceive no cause. But to attribute it to this cause would presuppose a permanent retention of the expanded state, of which I know no instance. The particular brittleness of hammered copper seems to show that the molecules are in quite a different state to those in annealed copper. It was to draw attention to this singular loss and recovery of density, that I have submitted these few facts to the Society.

XIX. — *On the Δ faced Polyacrons, in reference to the Problem of the Enumeration of Polyhedra.* *By* Mr. A. Cayley.

Read October 2nd, 1860.

The problem of the enumeration of polyhedra* is one of extreme difficulty, and I am not aware that it has been discussed elsewhere than in Mr. Kirkman's valuable series of papers on this subject in the *Memoirs* of the Society and in the *Philosophical Transactions.* A case of the general problem is that of the enumeration of the polyhedra with trihedral summits; and Mr. Kirkman in the earliest of his papers, viz. that "On the representation and enumeration of polyhedra," (*Memoirs,* vol. xii. pp. 47–70, 1854,) has in fact, by an examination of the particular case, accomplished the enumeration of the octahedra with trihedral summits. A subsequent paper "On the enumeration of x-edra having triedral summits and an $(x-1)$gonal base," (*Phil. Trans.* vol. xlvi. pp. 399–411, 1856,) relates, as the title shows, only to a special case of the problem of the polyhedra with trihedral summits, and in this particular case the number of polyhedra is more completely determined; but the later memoirs relate to the problem in all its generality, and the above-

* I use with Mr. Kirkman the expression "enumeration of polyhedra" to designate the general problem, but I consider that the problem is to find the different polyhedra rather than to count them, and I consequently take the word *enumeration* in the popular rather than the mathematical sense.

mentioned particular problem of the enumeration of the polyhedra with trihedral summits is not, I think, any where resumed. Instead of the polyhedra with trihedral summits, it is really the same thing, but it is rather more convenient to consider the polyacrons with triangular faces, or as these may for shortness be called, the Δ faced polyacrons; and it is intended in the present paper to give a method for the derivation of the Δ faced polyacrons of a given number of summits from those of the next inferior number of summits, and to exemplify it by finding, in an orderly manner, the Δ faced polyacrons up to the octacrons: thus, as regards the examples, stopping at the same point as Mr. Kirkman, for although perfectly practicable it would be very tedious to carry them further, and there would be no commensurate advantage in doing so. The epithet Δ faced will be omitted in the sequel, but it is to be understood throughout that I am speaking of such polyacrons only; and I shall for convenience use the epithets tripleural, tetrapleural, &c. to denote summits with three, four, &c. edges through them. The number of edges at a summit is of course equal to the number of faces, but it is the edges rather than the faces which have to be considered.

An n-acron has

$$n \text{ summits, } 3n-6 \text{ edges, } 2n-4 \text{ faces,}$$

and it is easy to see that there are the following three cases only, viz.:

1. The polyacron has at least one tripleural summit.
2. The polyacron, having no tripleural summit, has at least one tetrapleural summit.
3. The polyacron, having no tripleural or tetrapleural summit, has at least twelve pentipleural summits.

In fact, if the polyacron has c tripleural summits, d tetrapleural summits, e pentipleural summits, and so on, then we have

$$n = c + d + e + f + g + h + \&c.$$
$$6n - 12 = 3c + 4d + 5e + 6f + 7g + 8h + \&c.,$$

and therefore

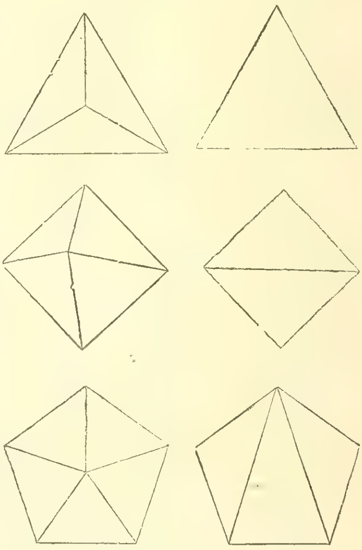
$$12 = 3c + 2d + e + 0f - g - 2h - \&c.,$$

or

$$3c + 2d + e = 12 + g + 2h + \&c.;$$

whence if $c = 0$ and $d = 0$, then $e = 12$ at least. It appears, moreover (since n cannot be less than e), that any polyacron with less than 12 summits cannot belong to the third class, and must therefore belong to the first or the second class.

An $(n+1)$-acron, by a process which I call the subtraction of a summit, may be reduced to an n-acron; viz., the faces about any summit of the $(n+1)$-acron stand upon a polygon (not in general a plane figure) which may be called the basic polygon, and when the summit with the faces and edges belonging to it is removed, the basic polygon, if a triangle, will be a face of the n-acron; if not a triangle, it can be partitioned into triangles which will be faces of the n-acron. The annexed figures exhibit the process for the cases of a tripleural, tetrapleural and pentipleural summit respectively, which are the only cases which need be considered; these may be called the first, second and third process respectively. It is proper to remark that for the same removed summit the first process can be performed in one



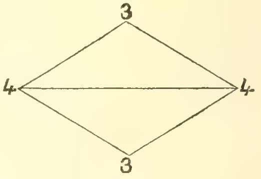
way only, the second process in two ways, the third in five ways; these being in fact the numbers of ways of partitioning the basic polygon.

We may in like manner, by the converse process of the addition of a summit, convert an n-acron into an $(n+1)$-acron; viz., it is only necessary to take on the n-acron a polygon of any number of sides, and make this the basic polygon of the new summit of the $(n+1)$-acron, and for this purpose to remove the faces within the polygon and substitute for them a set of triangular faces standing on the sides of the polygon and meeting in the new summit: the same figures exhibit the process for the cases of a tripleural, tetrapleural and pentipleural summit respectively, which (as for the subtractions) are the only cases which need be considered. It may be noticed that for the same basic polygon the process is in each case a unique one; the process is said to be the first, second, or third process, according as the new summit is tripleural, tetrapleural, or pentipleural.

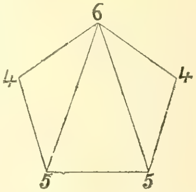
Now, reverting to the before-mentioned division of the polyacrons into three classes, an $(n+1)$-acron of the first class may by the first process of subtraction be reduced to an n-acron, and conversely it can be by the first process of addition derived from an n-acron. An $(n+1)$-acron of the second class, as having a tetrapleural summit, may by the second process of subtraction be reduced to an n-acron, and conversely it can be by the second process of addition derived from an n-acron. And in like manner, an $(n+1)$-acron of the third class, as having a pentipleural summit, may be by the third process of subtraction reduced to an n-acron, and conversely it may be by the third process of addition derived from an n-acron.

Hence all the $(n+1)$-acrons can be by the first, second and third processes of addition respectively derived from the n-acrons. It is to be observed that all the $(n+1)$-

acrons of the first class are obtained by the first process; the second process is only required for finding the $(n+1)$-acrons of the second class; and these being all obtained by means of it, the third process is only required for finding the $(n+1)$-acrons of the third class. Hence the second process need only be made use of when the n-acron has no tripleural summit, or when it has only one tripleural summit, or when, having two tripleural summits, they are the opposite summits of two adjacent faces. In the last-mentioned two cases respectively it is only necessary to consider the basic quadrangles which pass through the single tripleural summit and the basic quadrangle which passes through the two tripleural summits; for with any other basic quadrangle the derived $(n+1)$-acron would retain a tripleural summit, and would consequently be of the first class. The condition is more simply expressed as follows, viz.: The second process need only be employed when there is on the n-acron a basic quadrangle the summits of which are at least of the number of edges shown in the annexed figure, and all the other summits are at least 4-pleural.



Again, by the third process (as already mentioned) we seek only to obtain the $(n+1)$-acrons of the third class; the process need only be applied to the n-acrons for which there exists a basic pentagon the summits of which are at least of the number of edges shown in the annexed figure, all the other summits being at least 5-pleural; for it is only in this case that the derived $(n+1)$-acron will be of the third class.



The condition just referred to obviously implies that the n-acron is of the second or third class. It is to be noticed that in applying the foregoing principles

to the formation of the polyacrons as far as the 11-acrons we are only concerned with the first and second processes.

Consider the entire series of n-acrons, say A, B, C, &c., and suppose that the n-acron A gives rise to a certain number, say P, Q, R, S of $(n+1)$-acrons, the $(n+1)$-acron P is of course derivable from the n-acron A, but it may be derivable from other n-acrons, suppose from the n-acrons B and C. Then in considering the $(n+1)$-acrons derived from B, one of these will of course be found to be the $(n+1)$-acron P, and it is only the remaining $(n+1)$-acrons derived from B which are or may be $(n+1)$-acrons not already previously obtained as $(n+1)$-acrons derived from A. And if in this manner, as soon as each $(n+1)$-acron is obtained, we apply to it the process of subtraction so as to ascertain the entire series of n-acrons from which it is derivable, and, in forming the $(n+1)$-acrons derived from these, take account of the $(n+1)$-acrons already previously obtained and found to be derivable from these, we should obtain without any repetitions the entire series of the $(n+1)$-acrons.

For merely finding the number of the $(n+1)$-acrons, a more simple process might be adopted: say that an n-acron is p-wise generating when it gives rise to p $(n+1)$-acrons, and that it is q-wise generable when it can be derived from q $(n+1)$-acrons; and assume that a given n-acron is $(y_1+y_2+y_3+\&c.)$-wise generating, viz. that it gives rise to y_1 $(n+1)$-acrons which are 1-wise generable, y_2 $(n+1)$-acrons which are 2-wise generable, and so on; these forming the sum

$$\Sigma(y_1+\tfrac{1}{2}y_2+\tfrac{1}{3}y_3+\cdots)$$

where Σ refers to the entire series of the n-acrons, it is clear that every m-wise generable $(n+1)$-acron will in respect of each of the n-acrons from which it is derivable be reckoned as $\frac{1}{m}$, that is, it will be in the entire sum

reckoned as 1, and the sum in question will consequently be the number of the $(n+1)$-acrons.

The figures of the polyacrons comprised in the annexed Tables show the application of the method to the genesis of the polyacrons as far as the octacrons, in which the numbers indicate the nature of the different summits, according to the number of edges through each summit, viz., 3 a tripleural summit, 4 a tetrapleural summit, and so on. It will be noticed that there is only a single case in which this notation is insufficient to distinguish the polyacron, viz. among the octacrons there are two forms each of them with the same symbol 33445566; the inspection of the figures shows at once that these are wholly distinct forms, for in the first of them, viz. that derived from 3344555, each of the tripleural summits stands upon a basic triangle 456, while in the other of them, that from 3444555, each of the tripleural summits stands upon a basic triangle 566. But the symbol is merely generic, and of course in the polyacrons of a greater number of summits it may very well happen that a considerable number of polyacrons are comprised in the same genus.

The following remarks on the derivation of the octacrons from the heptacrons will further illustrate the method:

1. The heptacron 3335556 has three kinds of faces, viz. 355,* 356, 555, the first process consequently gives rise to 3 octacrons. As the heptacron has more than two tripleural summits the second process is not applicable.
2. The heptacron 3344466 has three kinds of faces, viz.: 366, 346 and 446, and the first process gives

* It is hardly necessary to remark that it must not be imagined that in general all the faces denoted by a symbol such as 355 (which determines only the nature of the summits on the face) are faces of the same kind, but this is so in the cases referred to in the text.

therefore 3 octacrons. The heptacron has only two tripleural summits, and they are disposed in the proper manner; the second process gives therefore 1 octacron.

3. The heptacron 3344556 has five kinds of faces, viz. 345, 346, 356, 456 and 455, and the first process consequently gives 5 octacrons. The heptacron has two tripleural summits, but they are not disposed in such manner as to render the second process applicable.
4. The heptacron 3444555 has four kinds of faces, viz. 355, 455, 445 and 444, and the first process gives therefore 4 octacrons. The heptacron has one tripleural summit, and the basic quadrangles 3545 which belong to it are of the same kind; the second process gives therefore 1 octacron.
5. The heptacron 4444455 has only one kind of face, viz. 445, and the first process gives therefore 1 octacron. There are two kinds of basic quadrangles, viz. 4545 and 4445, and the second process gives therefore 2 octacrons.

The number of octacrons would thus be 20, but by passing back from the octacrons to the heptacrons, it is found that there are in fact only 14 octacrons. Thus the octacron 33336666 has only one kind of tripleural summit 666 (the summit is here indicated by the symbol of the basic polygon) and the octacron is thus seen to be derivable from a single heptacron only, viz. the heptacron 3335556 from which it was in fact derived. But the octacron 33345567 has three kinds of tripleural summits, viz. 567, 557 and 467, and it is consequently derivable from three heptacrons, viz. the heptacrons 3335556, 3344466 and 3344555, and so on. The passage to the heptacrons from an octacron with one or more tripleural summits is of course always by the first process, but for the last two

octacrons, which have no tripleural summits, the passage back to the heptacrons is by the second process: thus for the octacron 44445555 we have but one kind of tetrapleural summit 4555; but as opposite pairs of summits of the basic quadrangle are of different kinds, viz. 45 and 55, we obtain two heptacrons, viz. 3444555 and 4444455. The octacron 44444466 has but one kind of tetrapleural summit, viz. 4646, and the pairs of opposite summits of the basic quadrangle being of the same kind 46, we obtain from it only the heptacron 4444455.

It may be remarked that for the five heptacrons respectively the values of the sum $y_1+\frac{1}{2}y_2+\frac{1}{3}y_3+\cdots$ are
$1+\frac{1}{3}+\frac{1}{2}$, $\frac{1}{3}+1+\frac{1}{2}+\frac{1}{2}$, $\frac{1}{3}+\frac{1}{2}+\frac{1}{2}+1+1$, $1+1+1+1+\frac{1}{2}$, $1+\frac{1}{2}+\frac{1}{2}$, giving for $\Sigma(y_1+\frac{1}{2}y_2+\frac{1}{3}y_3+\cdots)$ the value 14, as it should do.

XX.—*On a System of Periodic Disturbances of Atmospheric Pressure in Europe and Northern Asia.*

By Joseph Baxendell, F.R.A.S.

Read November 13th, 1860.

In the course of an investigation of the phenomena of the general disturbances of the atmosphere which I made some time ago, I was led to conclude that moderately accurate determinations of the sums of the irregular oscillations of the barometer for given periods, at different places on the surface of the earth, would afford valuable information respecting the nature of these disturbances, and at the same time throw additional light upon the causes by which they are produced. It is obvious that even the most accurate determinations of the *statical* element of mean pressure can be of only very limited use in an inquiry of this kind; but notwithstanding the importance of the subject, meteorologists have generally neglected to ascertain even approximately the values of the *dynamical* element as represented by the extent and frequency of the oscillations of the mercurial column. In none of the many volumes of observations which issue from the public observatories of this country and the continent have I yet seen any attempt made to deduce the values of this element. In the Greenwich, and also in some of the Oxford volumes, tables of the principal maximum and minimum readings of the barometer are given,

which it might be expected would be useful for determining readily the monthly and annual sums of the oscillations; but the systems on which these tables are formed at the two observatories are so widely different that while the sums of the oscillations for the years 1856 and 1857 by the Oxford tables amount to 70·95 inches and 70·62 inches, by the Greenwich tables they are only 58·22 inches and 56·22 inches respectively, the difference in the one year being 12·73 inches, and in the other 14·40 inches. In the Oxford volumes it appears from the heading of the tables that all oscillations above *one-tenth* of an inch are included; but in the Greenwich volumes it is not stated what limit has been adopted, and as the Greenwich barometric observations are not published in detail, it is impossible to ascertain with accuracy the maximum extent of the oscillations which have been excluded; and the results derived from the tables are therefore not available for comparison with those derived from observations made at other stations.

In the present state of practical meteorology accurate values of the barometric dynamical element can be obtained for those places only where hourly or bi-hourly observations are made. In by far the greater number of cases observations are made only twice, or at most four times a day; and the hours of observation are generally such as to render it difficult to separate readily the regular diurnal oscillations from the irregular and larger fluctuations which we are now considering. Under these circumstances I have thought it desirable to confine my attention, in the first instance at least, to oscillations derived from observations made once a day only. By this plan the regular diurnal oscillations are completely eliminated, the results obtained are more uniform, and better adapted for comparison with each other; and a greater number of sets of observations become available for the purposes of our

inquiry. I have accordingly employed this method in the discussion of a very considerable number of observations made at various places in Europe and Asia, and have obtained results which appear to me to possess considerable interest, as indicating very decidedly the existence of a remarkable and hitherto unsuspected law of disturbance of atmospheric pressure, which extends its influence over the greater portion of the European and a considerable portion of the Asiatic continent.

The following table contains the mean monthly and annual sums of the oscillations of the barometer at seven stations in Europe and six in Asia, as derived from observations extending over periods varying from six to fifteen years. The European stations are Dublin, Sandwick in Orkney, Greenwich, Milan, Stockholm, St. Petersburgh, and Lougan, 48°35′ N. 39°20′ E. The Asiatic stations are Tiflis, 40°42′ N. 44°50′ E.; Catherinbourg, 56°49′ N. 60°35′ E.; Barnaoul, 53°20′ N. 83°57′ E.; Irkoutzk, 52°17′ N. 103°35′ E.; Pekin, 39°54′ N. 116°27′ E.; and Nertchinsk, 51°19′ N. 119°36′ E. It must, however, be observed, with reference to the results given for Greenwich, that as the individual observations of each day at Greenwich are not given in the published volumes, these results are derived from the daily means, and not from single daily observations as in all the other cases:

Table of the Mean Monthly and Annual Sums of the Oscillations of the Barometer at seven stations in Europe and six in Northern Asia.

	1. Dublin. 11 years, 1843—9 and 1856—9.	2. Sandwick. 10 years, 1847 to 1856.	3. Greenwich. 10 years, 1848 to 1857.	4. Milan. 9 years, 1848 to 1856.	5. Stockholm. 10 years, 1847 to 1856.	6. St. Petersburgh. 8 years, 1848 and 1850—6.	7. Lougan. 8 years, 1848 and 1850—6.	8. Tiflis. 7 years, 1846—7 and 1852—6.	9. Catherinbourg. 10 years, 1846—8 and 1850—6.	10. Barnaoul. 10 years, 1846—8 and 1850—6.	11. Irkoutzk. 15 years, 1830 to 1844.	12. Pekin. 6 years, 1850 to 1855.	13. Nertchinsk. 8 years, 1848 and 1850—6.
	Inches												
January.....	6·60	7·45	6·08	4·46	6·11	5·92	4·91	3·59	5·33	5·35	5·02	4·06	3·79
February ...	5·32	6·54	5·00	4·78	7·27	6·94	4·93	3·62	5·13	4·94	3·57	4·30	4·05
March	6·03	5·81	5·07	4·49	6·03	6·41	5·53	4·11	5·60	6·68	5·28	4·81	4·61
April........	5·11	4·67	4·26	3·75	4·81	5·07	3·52	3·05	4·77	5·09	5·30	4·92	5·04
May	4·24	4·57	3·62	2·96	4·05	4·71	2·81	2·55	4·81	5·51	4·81	3·60	4·59
June.........	3·80	4·36	3·49	2·54	3·87	4·08	2·67	2·07	3·44	3·93	3·37	2·63	2·90
July	4·30	3·98	3·46	2·70	3·33	3·23	2·46	1·68	3·25	3·12	2·38	2·08	2·77
August	4·26	4·69	3·40	2·71	4·22	3·94	2·57	1·95	3·23	3·07	2·71	2·51	3·40
September ..	4·85	4·89	3·68	2·93	4·58	4·76	3·37	2·83	4·16	4·70	3·81	2·94	3·74
October	5·70	6·24	5·13	3·83	5·95	6·11	4·30	2·82	5·76	6·09	4·92	3·84	4·58
November...	5·34	6·46	5·10	4·15	5·99	6·69	4·54	3·22	5·67	6·56	5·39	4·22	4·74
December...	6·45	7·25	5·38	4·59	7·12	7·35	6·30	3·95	5·90	6·31	5·33	4·66	4·47
Year	62·00	66·91	53·67	43·89	63·33	65·21	47·91	35·44	57·05	61·35	51·89	44·57	48.68

1. *Proceedings of Royal Irish Academy* and *Journal of the Royal Dublin Society.* 2. *Lond. and Edinb. Phil. Magazine.* 3. *Greenwich Observations.* 4. *Giornale dell' I. R. Instituto Lombardo.* 5. *Öfversigt af Kongl. Svenska Vetensk. Akad. Förhandl.* 6, 7, 8, 9, 10, 11, 12 and 13, *Annales de l'Observatoire Physique Central de Russia.*

The curves laid down from the numbers in this table are exhibited in the plates which accompany this paper, and a slight inspection will show that in all of them a principal minimum occurs in one of the three summer months, June, July, or August, but generally in July, the warmest month of the year; and in many of them there is a second minimum occurring in one of the two winter months, January or February. With respect to the two maxima which occur between these minima, it will readily be observed that the interval between their summits gradually increases as we advance from the eastern to the western stations. Thus, at Nertchinsk, the first maximum occurs in the middle of April, and the second about the second week in November, the interval being nearly *seven* months; at Irkoutzk the first maximum occurs at the end of March or beginning of April, and the second maximum in the middle of November, the interval being *seven and a half* months; at Barnaoul the first maximum takes place in the middle of March, and the second in the middle of November, the interval being *eight* months; at Catherinbourg the first maximum occurs in the second week of March, and the second about the second week of December, the interval being *nine* months; at Tiflis, a station between the Black Sea and the Caspian, and at Lougan in southern European Russia, the interval is also about *nine* months; at St. Petersburgh the first maximum occurs in the third week of February, and the second in the second week of December, the interval being thus about *nine and a half* months; at Stockholm the first maximum occurs in the middle of February, and the second in the middle of December, the interval is therefore *ten* months; at Milan the interval is also about *ten* months; but at the western stations, Sandwick in Orkney, Greenwich and Dublin, we have only one principal maximum, which occurs about the second week of January, and which appears to be formed

by the union of the first maximum of one year with the second maximum of the year preceding, the interval between the two maxima being *twelve* months. It is evident, therefore, that these maxima move across the two continents in opposite directions, the course of the first being from west to east, and that of the second from east to west.

If these two maxima were produced by independent causes it might be expected that their joint action in the month of January would produce a compound maximum of much greater elevation than either of the separate maxima; but as this is not the case it appears to me very probable that both maxima are produced by the same disturbing cause, such disturbing cause taking its rise in Eastern Asia in the month of November, and gradually moving westward until it arrives in the British Islands in January; then, reversing its course, it returns with a diminished velocity to the region of its origin, where it arrives in the month of April, and afterwards rapidly disappears under the influence of an increasing temperature, to appear again later in the year on the return of a low temperature.

The middle latitude of the area over which these periodical disturbances of atmospheric pressure take place is about 51°, and the difference of longitude of the extreme east and west stations being about 126°, it follows that the length of this area is more than four thousand six hundred geographical miles; and as the difference of latitude between the extreme north and south stations in Eastern Europe and Western Asia is more than 19°, its breadth must considerably exceed eleven hundred miles.

With reference to the probable nature and origin of the disturbing cause it may be remarked that the times of its first appearance and final disappearance in Eastern and Central Asia correspond very nearly with the times of the

breaking up of the periodical trade winds or monsoons in the China Sea and Indian Ocean. It is probable, therefore, that the two systems of phenomena are directly connected with and dependent upon each other; and as the changes of the monsoons are known to be sometimes very irregular it will become an interesting subject of inquiry to ascertain whether corresponding irregularities occur in the disturbances of the atmosphere in the middle and higher latitudes of Europe and Asia.

Although probably not bearing directly upon the subject of this paper, I may perhaps be allowed to draw attention to a very decided *convexity* of nearly all the curves in the month of October, in two or three cases producing a small maximum. Indications of a similar irregularity in the month of March are also apparent in some of the curves. These features clearly indicate the operation of a secondary disturbing cause acting during those months, but more especially during October, over the whole breadth of the two continents, and therefore probably differing altogether in its nature and origin from the disturbing cause which produces the two great movable maxima.

It may be well to state that the mean monthly values given in the table have not been corrected for the differences in the lengths of the months; but corrected values would not materially affect the general results, and it was not considered necessary in a preliminary inquiry to adopt months of equal length, or to calculate the mean daily amount of oscillation for each month.

In concluding this brief account of results which seem to me to indicate clearly the existence of a regular law of atmospheric disturbance in regions of the earth which have hitherto been generally regarded as remarkable for the apparent irregularity of their atmospherical phenomena, I cannot omit to mention the fact that the first application of the method of reduction which has led me to these

results appears to be due to Dr. Dalton, who, at page 16 of the second edition of his *Meteorological Essays*, gives a little table entitled "A table of the mean spaces described by the mercury each month, determined by summing up the several small spaces ascended and descended; also the mean number of changes from ascent to descent, and the contrary, each month, it being reckoned a change when the space described is upwards of ·03 of an inch. The means are for five years, at Kendal and Keswick." There is no remark appended to this table, but in another part of the work the author refers to it in support of his theory of the variation of the barometer as showing that the greatest disturbance of atmospheric pressure takes place in the coldest period of the year, and having apparently satisfied himself on this point it would seem that he never afterwards resumed the subject, and it has since remained almost entirely neglected by meteorologists.

The two sets of numbers in Dr. Dalton's table indicate a maximum of disturbance in the first week of January, thus agreeing very closely with the results which I have obtained for Dublin, Sandwick, and Greenwich.

XXI.—*On the Irregular Oscillations of the Barometer at Manchester.*

By G. V. Vernon, F.R.A.S.

Read April 2nd, 1861.

A paper having recently been read before the Society by Mr. Baxendell, upon the values of the mean monthly and annual amounts of the irregular oscillations of the barometer, at various localities upon the globe, it was thought that similar deductions from observations made at Manchester might be acceptable as additional data upon the subject.

All the observations given in this paper were made with standard instruments. The barometer readings used were made daily at eight a.m.

In Table I. are given the total monthly amounts of the oscillations and their number.

The number of oscillations in each month may be slightly in error, but never to the extent of an entire oscillation: this arises from oscillations beginning in one month and ending in another. As the number of oscillations could not be deduced for the entire year, it was not thought necessary to estimate this number more closely.

Table II. contains the mean daily oscillations for each month, and has been formed by dividing the figures in Table I. by the number of days in each month.

The maximum amount of oscillation appears to take place in January and the minimum in July, to which may

be added a second maximum which appears to take place in October.

Some observations, kindly furnished me by Mr. John Curtis, give the following values for the month of August during the last six years:

	Amount of Oscillations.	No.
1855 ...	0·146 ...	16
1856 ...	0·126 ...	18
1857 ...	0·095 ...	11
1858 ...	0·102 ...	15
1859 ...	0·115 ...	12
1860 ...	0·159 ...	16
Means ...	0·124 ...	1·47

The mean daily oscillations, and their number, are then as follows:

	Mean daily Amount. Inches.	No.
January ...	0·226 ...	14·9
February ...	0·193 ...	14·6
March ...	0·191 ...	14·6
April ...	0·166 ...	14·6
May ...	0·135 ...	14·1
June ...	0·131 ...	14·2
July ...	0·121 ...	15·3
August ...	0·124 ...	14·7
September ...	0·151 ...	13·9
October ...	0·193 ...	15·7
November ...	0·182 ...	13·2
December ...	0·206 ...	15·7
Mean for the year	0·168	14·63
Total for the year	61·41	175·5

The August values being from six years' observations only, I have not tabulated them in Tables I. and II., as additional years might alter the values to some extent.

In the diagram, I have added the curve for Dublin from the reductions given by Colonel Sir Henry James in the volume of observations made at the Ordnance Office, Dublin. The features of the two curves are very similar. The Dublin curve, however, does not show so low a minimum as the Manchester one; it gives a somewhat higher maximum, and also higher values, in the months of February and October. The two curves being for different

periods, of course somewhat interferes with the exact comparison.

Table III. contains the fall of rain for each month compared with my own average for twelve years and Dr. Dalton's average for forty-seven years.

Table IV. contains the mean monthly temperatures and their difference from the twelve years' average. It is much to be regretted that we have no trustworthy observations of the temperature for as long a period as that of the fall of rain, as differences from a longer average would have been of more value for this part of the investigation.

Having obtained the values of the oscillations, it was thought desirable to determine what connection there might be between the amount of oscillation and the fall of rain; also the relation, if any, existing between the mean temperature and the amount of oscillation.

In Table V. the falls of rain above the average are compared with the corresponding amounts of oscillation, and the falls of rain below the average with their corresponding oscillations. This table shows that a fall of rain in excess of the average, is attended by a considerable increase in the amount of the oscillations in every month but October; and it is very probable that this month would agree with the others, if taken from a longer series of observations.

In Table VI. similar data are given for the mean temperatures of each month, according as they are above or below the average.

In this case we find that in February, April, May, June, July, October and December, a temperature below the mean for the month increases the amount of the oscillations, whilst in the remaining months of January, March, August, September and November the converse would appear to hold good.

In Table VII. the numbers of oscillations above or below the average are compared with the corresponding falls of

rain in precisely the same manner as in the two preceding tables.

We find from this table, that a number of oscillations above the average is accompanied by a larger fall of rain than that which accompanies a number of oscillations below the average in the months of January, February, March, June, October, November and December. In the remaining months of the year, April, May, July and September, a number of oscillations in excess of the average is accompanied by a diminished fall of rain.

Leaving out of consideration the month of June as being perhaps abnormal (observations for this month in 1851, 1857, 1859 and 1860 being also wanting), it would appear that we have two different laws, one for the six winter months and another for the six summer months,—one of these laws being the direct converse of the other.

The mean for the eleven months gives the following:

Number of Oscillations above or below the average.		Fall of Rain. Inches.
+ 1·96	...	2·509
— 2·02	...	2·359

This seems to show that on the mean of the entire year, the number of oscillations does not seem to affect the fall of rain, but only appears to do so when the separate months or seasons of the year are taken separately.

The apparent or real relation which appears to exist amongst these various data, seems well worth further investigation with the aid of a longer series of observations.

TABLE I.—Monthly Amounts and Numbers of the Irregular Oscillations of the Barometer at Manchester, from Observations made daily at 8h. a.m. by G. V. Vernon, F.R.A.S.

	January.		February.		March.		April.	
Year	Oscilltns	No.	Oscilltns	No.	Oscilltns	No.	Oscilltns	No.
	Inches.		Inches.		Inches.		Inches.	
1849	...	...	...	...	5·402	15	4·856	19
1850	7·532	11	6·322	15	4·479	16	6·537	14
1851	6·965	15	4·584	14	5·715	18	Incom	plete
1852	10·961	17	7·320	14	3·991	12	3·497	16
1853	6·122	18	5·677	14	5·794	17	4·986	15
1854	6·275	16	5·818	15	5·517	15	4·896	15
1855	4·036	19	4·742	14	8·020	11	4·923	12
1856	7·249	12	3·871	20	3·282	14	4·512	14
1857	8·495	11	4·040	14	7·613	14	5·222	17
1858	5·315	16	3·770	13	6·164	11	5·544	14
1859	4·832	13	5·938	15	8·186	14	5·336	15
1860	9·141	16	8·620	13	6·995	20	4·626	10
Means	6·993		5·518		5·929		4·994	

	May.		June.		July.		August.	
Year	Oscilltns	No.	Oscilltns	No.	Oscilltns	No.	Oscilltns	No.
	Inches.		Inches.		Inches.			
1849	4·727	14	3·621	17	Incom	plete	...	...
1850	3·904	14	4·983	13	3·131	17	...	...
1851	3·885	11	Incom	plete	5·407	17	...	...
1852	3·764	15	4·040	18	3·089	16	...	...
1853	4·423	11	2·888	12	4·446	15	...	...
1854	4·911	14	3·369	11	3·103	16	...	...
1855	4·729	19	4·823	13	4·063	14	...	...
1856	5·478	12	4·868	15	3·804	13	...	...
1857	3·381	13	Incom	plete	4·265	16	...	...
1858	Incom	plete	2·806	15	4·119	12	...	...
1859	2·551	18	Incom	plete	2·637	18	...	...
1860	Incom	plete	Incom	plete	3·179	14	...	...
Means	4·175		3·925		3·749			

	September.		October.		November.		December.	
Year	Oscilltns	No.	Oscilltns	No.	Oscilltns	No.	Oscilltns	No.
	Inches.		Inches.		Inches.		Inches.	
1849	5·011	11	7·351	16	6·349	11	7·322	15
1850	3·503	11	6·535	16	7·708	16	5·061	11
1851	4·108	14	5·006	12	4·909	14	4·210	14
1852	4·939	13	5·856	16	6·105	14	8·572	22
1853	4·365	10	Incom	plete	4·398	16	5·270	16
1854	3·386	15	7·333	18	8·083	13	8·040	21
1855	3·805	18	5·913	16	4·160	13	6·014	17
1856	5·290	14	4·439	14	4·568	11	8·128	17
1857	Incom	plete	5·550	13	4·882	13	4·750	15
1858	6·234	15	5·776	14	4·247	10	5·765	18
1859	5·292	17	6·116	19	5·174	13	7·407	13
1860	4·907	15	5·961	19	4·888	15	6·143	9
Means	4·530		5·985		5·456		6·390	

TABLE II.—Monthly Mean Daily Amount of Oscillation of the Barometer at Manchester.

Year	January.	February.	March.	April.	May.	June.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
1849	...	...	0·174	0·162	0·152	0·121
1850	0·243	0·226	0·144	0·218	0·126	0·166
1851	0·225	0·163	0·184	...	0·125	...
1852	0·354	0·252	0·129	0·117	0·121	0·135
1853	0·197	0·203	0·187	0·166	0·143	0·096
1854	0·202	0·208	0·178	0·163	0·158	0·112
1855	0·130	0·169	0·259	0·164	0·153	0·161
1856	0·234	0·133	0·106	0·150	0·177	0·163
1857	0·274	0·144	0·246	0·174	0·109	...
1858	0·171	0·121	0·199	0·185	...	0·093
1859	0·156	0·212	0·264	0·178	0·082	...
1860	0·295	0·297	0·226	0·154	...	...
Means	0·226	0·193	0·191	0·166	0·135	0·131
Mean No. of Oscillations for each month.	14·9	14·6	14·6	14·6	14·1	14·2

Year	July.	August.	September.	October.	November.	December.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
1849	...	...	0·167	0·237	0·212	0·236
1850	0·101	...	0·117	0·211	0·257	0·163
1851	0·174	...	0·137	0·161	0·164	0·136
1852	0·100	...	0·165	0·189	0·204	0·277
1853	0·143	...	0·146	...	0·147	0·170
1854	0·100	...	0·113	0·237	0·269	0·259
1855	0·131	...	0·127	0·191	0·139	0·194
1856	0·123	...	0·176	0·143	0·152	0·262
1857	0·138	...	...	0·179	0·163	0·153
1858	0·133	...	0·174	0·186	0·142	0·186
1859	0·085	...	0·176	0·197	0·172	0·239
1860	0·103	...	0·164	0·192	0·163	0·198
Means	0·121	...	0·151	0·193	0·182	0·206
Mean No. of Oscillations for each month.	15·3	...	13·9	15·7	13·2	15·7

TABLE III.—Monthly Fall of Rain at Manchester, and Differences from the Means.

	January.			February.			March.			April.			May.			June.		
Year	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.
	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches
1849	...	...	...	...	...	...	...	...	...	...	...	...	...	..	...	...	...	...
1850	...	...	...	...	...	...	0·886	− 1·039	− 1·422	2·703	+ 0·855	+ 0·589	1·529	− 0·279	− 0·917	1·400	− 1·910	− 1·291
1851	3·396	+ 0·618	+ 1·139	1·431	− 0·468	− 1·012	3·243	+ 1·318	+ 0·935	...	...	...	1·996	+ 0·188	− 0·450	...	...	...
1852	5·165	+ 2·387	+ 2·908	3·965	+ 2·066	+ 1·522	0·456	− 1·469	− 1·852	0·522	− 1·323	− 1·592	1·417	− 0·391	− 1·029	4·710	+ 1·400	+ 2·019
1853	2·742	− 0·036	+ 0·485	0·893	− 1·006	− 1·550	1·963	+ 0·038	− 0·345	1·713	− 0·135	− 0·401	1·420	− 0·388	− 1·026	4·951	+ 1·641	+ 2·260
1854	2·996	+ 0·218	+ 0·739	2·867	+ 0·968	+ 0·424	1·458	− 0·467	− 0·850	0·665	− 1·183	− 1·449	1·668	− 0·140	− 0·778	1·727	− 1·583	− 0·964
1855	0·765	− 2·013	− 1·492	1·449	− 0·450	− 0·994	2·018	+ 0·093	− 0·290	1·005	− 0·843	− 1·109	1·526	− 0·282	− 0·920	3·212	− 0·098	+ 0·521
1856	2·745	− 0·033	+ 0·488	2·799	+ 0·900	+ 0·356	0·197	− 1·728	− 2·111	2·518	+ 0·670	+ 0·404	2·881	+ 1·073	+ 0·435	3·373	+ 0·063	+ 0·682
1857	3·121	+ 0·343	+ 0·864	1·932	+ 0·033	− 0·511	2·110	+ 0·185	− 0·198	2·189	+ 0·341	+ 0·075	2·207	+ 0·399	− 0·239	2·215	− 1·095	− 0·476
1858	1·504	− 1·274	− 0·753	0·325	− 1·574	− 2·118	1·940	+ 0·015	− 0·368	2·714	+ 0·866	+ 0·600	2·105	+ 0·297	− 0·341	3·181	+ 0·071	+ 0·490
1859	1·760	− 1·018	− 0·497	2·456	+ 0·557	+ 0·013	3·452	+ 1·530	+ 1·144	3·143	+ 1·295	+ 1·029	0·452	− 1·356	− 1·994	2·291	− 1·019	− 0·400
1860	3·584	+ 0·806	+ 1·327	0·873	− 1·026	− 1·570	3·471	+ 1·546	+ 1·163	1·306	− 0·542	− 0·808	2·684	+ 0·876	+ 0·238	6·039	+ 2·729	+ 3·348
Mns	2·778			1·899			1·925			1·848			1·808			3·310		

	July.			August.			September.			October.			November.			December.		
Year	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.	Totals	12 years.	47 years.
	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches
1849	...	...	...	...	...	..	...	...	...	...	...	...	...	...	...	...	...	...
1850	1·730	− 1·139	− 1·976	...	. .	...	2·653	− 0·092	− 0·539	2·885	− 0·395	− 0·869	4·036	+ 1·425	+ 0·324	2·177	− 0·609	− 1·260
1851	3·372	+ 0·503	− 0·334	...	...	...	1·312	− 1·433	− 1·880	3·921	+ 0·641	+ 0·167	1·861	− 0·750	− 1·851	1·378	− 1·408	− 2·059
1852	2·834	− 0·035	− 0·872	...	...	...	2·400	− 0·345	− 0·792	2·685	− 0·595	− 1·069	4·481	+ 1·870	+ 0·769	4·894	+ 2·108	+ 1·457
1853	3·644	+ 0·775	− 0·062	...	...	...	3·644	+ 0·899	+ 0·452	...	...	...	...	...	...	0·863	− 1·923	− 2·574
1854	2·852	− 0·017	− 0·854	...	...	...	2·369	− 0·376	− 0·823	3·205	− 0·075	− 0·549	3·122	+ 0·511	− 0·590	5·753	+ 2·967	+ 2·316
1855	4·245	+ 1·376	+ 0·539	...	...	...	0·963	− 1·782	− 2·229	5·208	+ 1·928	+ 1·454	0·631	− 1·980	− 3·081	1·060	− 1·726	− 2·377
1856	3·045	+ 0·176	+ 0·661	...	...	...	3·178	+ 0·433	− 0·014	2·318	− 0·962	− 1·436	3·865	+ 1·254	+ 0·153	3·594	+ 0·808	+ 0·157
1857	4·546	+ 1·677	+ 0·840	4·733	...	+ 1·270	3·146	+ 0·401	− 0·046	1·734	− 1·546	− 2·020	1·558	− 1·053	− 2·154	1·520	− 1·266	− 1·917
1858	2·320	− 0·549	− 1·386	3·330	...	− 0·133	3·067	+ 0·322	− 0·125	4·044	+ 0·764	+ 0·290	1·335	− 1·276	− 2·377	3·569	+ 0·783	+ 0·132
1859	1·314	− 1·555	− 2·392	5·991	...	+ 2·528	4·589	+ 1·844	+ 1·397	3·372	+ 0·092	− 0·382	3·101	+ 0·490	− 0·611	2·574	− 0·212	− 0·863
1860	1·660	− 1·209	− 2·046	5·172	...	+ 1·709	2·875	+ 0·130	− 0·317	3·428	+ 0·148	− 0·326	2·117	− 0·494	− 0·595	3·261	+ 0·475	− 0·176
Mns	2·869						2·745			3·280			2·611			2·786		

TABLE IV.—Mean Monthly Temperatures at Manchester, and Differences from the Means.

Year	January.		February.		March.		April.		May.		June.	
	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.
	°	°	°	°	°	°	°	°	°	°	°	°
1849	...	...	...	...	42·8	+1·5	44·7	−1·9	53·3	+1·5	56·3	−1·2
1850	34·4	−3·9	44·1	+6·3	40·8	−0·5	48·7	+2·1	50·5	−1·3	58·6	+1·1
1851	41·4	+3·1	39·4	+1·6	42·5	+1·2	...	...	50·7	−1·1	...	...
1852	40·0	+1·7	40·0	+2·2	41·1	−0·2	48·0	+1·4	52·7	+0·9	58·6	+1·1
1853	40·6	+2·3	32·8	−5·0	38·2	−3·1	45·6	−1·0	51·6	−0·2	58·6	+1·1
1854	37·7	−0·6	39·0	+1·2	44·0	+2·7	49·4	+2·8	52·3	+0·5	57·1	−0·4
1855	36·7	−1·6	28·6	−9·2	38·2	−3·1	46·1	−0·5	48·5	−3·3	57·1	−0·4
1856	37·7	−0·6	41·1	+3·3	41·1	−0·2	47·2	+0·6	50·2	−0·6	55·7	−1·8
1857	36·5	−1·8	39·3	+1·5	41·8	+0·5	46·4	−0·2	52·7	+0·9	61·2	+3·7
1858	37·9	−0·4	35·3	−2·5	40·6	−0·7	46·4	−0·2	49·9	−2·1	63·3	+5·8
1859	41·1	+2·8	41·1	+3·3	44·8	+3·5	45·2	−1·4	54·3	+2·5	60·2	+2·7
1860	37·7	−0·6	34·5	−3·3	39·8	−1·5	44·4	−2·2	54·5	+2·7	55·2	−2·3
Mns	38·3		37·8		41·3		46·6		51·8		57·5	

Year	July.		August.		September.		October.		November.		December.	
	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.	Mean	Diffnc.
	°	°	°	°	°	°	°	°	°	°	°	°
1849	59·8	−0·7	...	...	56·1	+0·9	48·1	−0·4	43·9	+2·7	38·6	−0·3
1850	60·4	−0·1	...	...	53·9	−1·3	45·6	−2·9	44·9	+3·7	39·5	+0·6
1851	58·2	−2·3	...	...	53·6	−1·6	50·5	+2·0	35·6	−5·6	40·7	+1·8
1852	67·9	+7·4	...	...	54·3	−0·9	44·6	−3·9	45·0	+3·8	45·2	+6·3
1853	58·6	−1·9	...	...	54·1	−1·1	...	...	41·7	+0·5	35·2	−3·7
1854	60·3	−0·2	...	...	57·2	+2·0	47·0	−1·5	40·3	−0·9	39·8	+0·9
1855	61·8	+1·3	...	...	54·9	−0·3	48·5	0·0	41·5	+0·3	35·6	−3·3
1856	58·7	−1·8	...	...	54·2	−1·0	51·8	+3·3	39·2	−2·0	38·5	−0·4
1857	60·6	+0·1	62·9	...	57·7	+2·5	52·0	+3·5	43·8	+2·6	45·2	+6·3
1858	57·9	−2·6	61·7	...	59·2	+4·0	48·2	−0·3	39·2	−2·0	40·8	+1·9
1859	64·3	+3·8	...	...	55·2	0·0	48·5	0·0	39·6	−1·6	34·1	−4·8
1860	57·6	−2·9	...	...	51·7	−3·5	48·6	+0·1	40·0	−1·2	34·5	−4·4
Mns	60·5				55·2		48·5		41·2		38·9	

TABLE V.—Excess and Deficit of Rain compared with the Amount of the Oscillations of the Barometer.

Month.	Excess of Rain.	Amount of Oscillations.	Deficit of Rain.	Amount of Oscillations.
	+ Inches	Inches	— Inches	Inches
January......	1·136	7·887	0·914	4·727
February ...	0.579	5·737	1·292	5·239
March	1·081	6·965	0·929	5·607
April	0·539	5·346	0·893	4·501
May	0·435	5·478	0·919	3·943
June	1·194	3·885	1·127	3.676
July	0·680	4·044	1·240	3.639
August	...	...	...	...
September...	0·924	4·828	0·752	4·396
October......	0·637	5·565	0·950	5·970
November ...	0·415	6·127	1·608	5·192
December ...	1·016	7·626	1·604	5·551
Means ...	0·785	5·772	1·111	4·822

TABLE VI.—Excess and Deficit of Mean Temperature compared with the amount of the Oscillations of the Barometer.

Month.	Excess of Temperature.	Amount of Oscillations.	Deficit of Temperature.	Amount of Oscillations.
	+ °	Inches	— °	Inches
January......	2·48	7·220	1·36	6·863
February ...	2·77	5·413	5·00	5·702
March	1·88	6·487	1·33	5·532
April	1·72	4·861	1·06	5·071
May	1·26	3·867	1·30	4·484
June	2·28	3·679	0·95	4·170
July	3·15	3·513	1·68	3·884
August	...	...	...	...
September...	1·72	4·731	1·21	4·526
October......	1·45	5·497	1·28	6·414
November...	2·26	5·600	2·21	5·311
December ...	2·97	6·066	2·81	6·714
Means ...	2·18	5·176	1·74	5·334

TABLE VII.—Monthly Oscillations above and below the average number, compared with the corresponding Falls of Rain.

Month.	Oscillations in Excess.	Rain.	Oscillations in Deficit.	Rain.
	+	Inches	—	Inches
January......	1·81	2·879	3·40	2·542
February ...	1·65	2·707	0·88	1·553
March	2·23	2·204	1·93	1·696
April	1·57	1·646	1·80	2·049
May	2·90	1·132	1·38	1·950
June	2·47	3·754	1·95	2·822
July	1·37	2·775	1·70	2·983
August	...	...	...	...
September...	1·53	2·622	2·65	2·899
October......	1·44	3·464	2·45	3·004
November...	1·80	3·124	1·20	2·269
December ...	2·81	3·289	2·87	2·182

XXII. — *On the Theory of Groups and many-valued Functions.*

By the Rev. Tho. P. Kirkman, M.A., F.R.S.

Read April 16th, 1861.

§ 1.

First principles: Factors of a substitution: Permutable substitutions.

1. Let G denote the sum of k different arrangements

$$A_1 A_2 \cdot \cdot A_k$$

taken from the $1 \cdot 2 \cdot 3 \cdot \cdot N$ permutations of the N elements

$$1, 2, 3 \cdot \cdot N-1 \cdot N.$$

Every pair $A_m\ A_n$ of these k permutations gives a substitution, which is written

$$\frac{A_m}{A_n},$$

and which has reference to a subject B, on which we operate. This subject is any permutation of the N elements. The effect of the substitution on B is to exchange in B, for any letter a, that which stands above a in the substitution. We express that C is the result by writing

$$\frac{A_m}{A_n} B = C.$$

If the k arrangements are such that every result of operation by any substitution made with any pair on any arrangement is an arrangement of the system, they form a *group of permutations.*

2. It is sufficient to consider groups of which an arrangement is the natural order $1\,2\,3 \cdot\cdot N$ of the elements, which is called unity, and may be denoted by (1). Such a group is

$$1 A_1 A_2 \cdot\cdot A_{k-1}.$$

Every substitution in this group may be written with unity for denominator; that is, we have always

$$\frac{A_m}{A_n} = \frac{A_r}{(1)},$$

A_r being one of the k arrangements. And the k substitutions of the group are

$$\frac{(1)}{(1)}, \frac{A_1}{(1)}, \frac{A_2}{(1)} \cdot\cdot \frac{A_{k-1}}{(1)},$$

which may be written

$$(1) A_1 A_2 \cdot\cdot A_{k-1}.$$

A group of k permutations, of which one is unity, is a *model group of the order k.*

The simplest definition of a group is this — that *the product of any two substitutions of the group is a substitution of the group*; that is, in any model group,

$$A_p\, A_q = A_r$$
$$A_p\, A_p = A_p^2 = A_s,$$

A_r and A_s being in the group. It follows that every power of a substitution is in the group.

If $$A_p^b = 1,$$

we say that A_p *is a substitution of the b^{th} order.* We have also

$$A_p^{b-c} = A_p^{-c};$$

that is, every negative power of any substitution is one of the group. In fact,

$$\frac{A_p^{-c}}{1} = \frac{1}{A_p^c} = \frac{1}{A_t} = \frac{A_v}{1}.$$

3. It is important to be able to form readily the product of two substitutions A_m and A_n in a model group. The simple rule is: *Pronounce the arrangement* A_m *with*

which you operate on A_n, *and at the same time write down the consecutive elements of* A_m *in the same order in which you see* $1\,2\,3 \cdot \cdot N$ *disposed in* A_n.

For example, in the group

$$\begin{array}{c} 12345 \\ 31452 \\ 43521 \\ 54213 \\ 25134, \end{array}$$

we have

$$\begin{aligned} 31452 \cdot 31452 &= 43521 = (31452)^2 \\ 31452 \cdot 43521 &= 54213 = (31452)^3 \\ 31452 \cdot 54213 &= 25134 = (31452)^4 \\ 31452 \cdot 25134 &= 12345 = (31452)^5 = (1) \\ 31452 \cdot 12345 &= 31452 = 31452. \end{aligned}$$

4. Every substitution A_m is obtained by operating on unity with cyclical permutations of certain circles of elements, which are called the *circular factors* of the substitution. For example, the substitutions

$$654213, \; 312564, \; 465132,$$

may be written

$$\frac{634521}{163452}(1), \; \frac{321,564}{132,456}(1), \; \frac{41,62,53}{14,26,35}(1),$$

of which the first has a circular factor of six elements; the second has two factors each of three elements; and the third has three factors of two elements.

It is usual to say that the first has a factor of the sixth order, the second has two factors of the third order, and that the third has three factors of the second order. We speak also of the order of the substitution θ, which is the number of its different powers, $(1\,\theta\,\theta^2 \cdot \cdot \cdot \cdot)$.

It is well known that the *order of a substitution is the least common multiple of the order of its circular factors.*

It is useful to be able to see the circular factors without losing sight of the form of the substitution in a model

group. We may write the circular factors under the first of their elements which occurs in the substitution thus:

$$\underset{\substack{2\\1}}{3}\,1\,2\,\underset{\substack{6\\4}}{5}\,6\,4,\quad \underset{1}{4}\,\underset{2}{6}\,\underset{3}{5}\,1\,3\,2$$

$$\underset{\substack{8\\6\\4\\1}}{3}\,\underset{2}{5}\,8\,1\,2\,4\,\underset{\substack{c\\7}}{0}\,6\,\underset{\substack{a\\9}}{b}\,c\,9\,a\,7\,.$$

In the notation of Cauchy the last substitution is represented thus:

$$(1\,3\,8\,6\,4)\,(7\,0\,c)\,(9\,a\,b)\,(2\,5),$$

of which the inconvenience is that we lose sight of the form which the substitution wears, viz.,

$$3\,5\,8\,1\,2\,4\,0\,6\,b\,c\,9\,a\,7,$$

in a model group.

5. The product of two substitutions PQ varies in general with the written order of the two factors. But in some cases we have

$$\mathrm{PQ}=\mathrm{QP}.$$

When this is true we say that Q *is permutable with* P.

If we change in any way the sequence of the circular factors of the same order, and write the changed sequence over the given one, we have always a substitution permutable with the given one. Thus let the given one be

$$\underset{\substack{6\\1}}{3}\,\underset{\substack{5\\2}}{9}\,6\,\underset{\substack{0\\4}}{8}\,2\,1\,\underset{7}{a}\,0\,5\,4\,7\,\underset{b}{c}\,b=\mathrm{P}$$

made with thirteen elements.

We can form Q and Q′, both permutable with P, thus:

$$\frac{804\cdot 361}{361\cdot 804}=4\,2\,8\,1\,5\,0\,7\,3\,9\,6\,a\,c\,b=\mathrm{Q}$$

$$\frac{804\cdot 361\cdot cb\cdot a7}{361\cdot 804\cdot a7\cdot cb}=4\,2\,8\,1\,5\,0\,b\,3\,9\,6\,c\,7\,a=\mathrm{Q}'$$

and we have

$$\mathrm{QP}=\mathrm{PQ}$$

$$\mathrm{Q'P}=\mathrm{PQ'},$$

as it may be proved by the rule given in (3).

This is a theorem of Cauchy's, whose somewhat difficult

demonstration may be seen in his "Mémoire sur les arrangements que l'on peut former avec des lettres données," *Exercises d'Analyse et de Physique Mathématique, tome troisième.* And its truth will be more simply evident by what follows on the construction of groups.

§ 2.

Deranged groups: Derived groups.

6. Let G be a model group of k substitutions

$$1 A_1 A_2 \cdot \cdot A_{k-1},$$

and let P be any substitution not found in G.

We can write the product

$$\begin{aligned} GP = {} & P \\ & A_1P \\ & A_2P \\ & \vdots \\ & A_{k-1}P \end{aligned}$$

in a vertical column. None of the k arrangements thus made is in G; for if

$$A_mP = A_n$$

we have

$$P = A_m^{-1} A_n = A_p, \text{ by } (2);$$

that is, P is one of the substitutions of G, contrary to hypothesis. The only effect of P on G is to change the arrangement of entire vertical columns of G, as is evident if we compare together the products

$$(1)P,\ A_1P,\ A_2P,\ \&c.$$

We see that the m^{th} element of every arrangement in G, by the rule of Art. 3, is placed in the same vertical row.

We shall call GP *the derangement of* G *by* P. It is known, and easily proved, that the derangement GP of G by P is identical with the derangement of G by A_mP, A_m being any substitution of G.

Let Q be an arrangement of the N elements which is

neither in G nor in GP. Then no arrangement in GQ can be G; nor can it be in GP; for if

$$A_m P = A_n Q,$$

we should have

$$A_n^{-1} A_m P = Q = A_r P;$$

that is, Q is in GP, contrary to hypothesis.

Hence we see that G, GP, and GQ are what Betti has called *equal groups*, that is, groups of permutations, all whose substitutions are identical, the common model being G.

In this way we can partition the entire system of $1 \cdot 2 \cdot 3 \cdot \cdot N$ permutations of N elements into

$$\frac{1 \cdot 2 \cdot 3 \cdot \cdot N}{k}$$

equal groups, by adding to

$$G, GP, GQ \cdot \cdot$$

the derangement GR of G by R, which is not in $G + GP + GQ$.

Hence we see that G has $\frac{\Pi N}{k} - 1$ derangements, and that it has no more, and that these are all *equal groups*.

7. Let G be a model group of the order k, made with N elements, viz.,

$$G = 1 + A_1 + A_2 + \cdot \cdot + A_{k-1};$$

and let P be any substitution not in G.

The product

$$PG = P + PA_1 + PA_2 + \cdot \cdot + PA_{k-1}$$

is a group of substitutions. In fact it is the derangement by P of the model group

$$PGP^{-1} = 1 + PA_1P^{-1} + PA_2P^{-1} + \cdot \cdot,$$

which is a group, because, according to the definition (2),

$$PA_m P^{-1} PA_n P^{-1} = PA_m A_n P^{-1} = PA_q P^{-1}.$$

The group PQ is *the derivate of* G *by* P.

No arrangement of PG is in G; for if

$$A_m = PA_n,$$

we should have

$$A_m A_n^{-1} = P = A_r,$$

or P would be in G, contrary to hypothesis.

If Q be neither in G nor in PG, no arrangement in QG can be in G or in PQ; for if

$$QA_m = PA_n,$$
$$Q = PA_m A_n^{-1} = PA_s,$$

contrary to hypothesis.

If then we add to $G + PQ + QG \cdot\cdot$ the derivate RG by R, which is not in the preceding groups, we shall *partition the* ΠN *permutations of* N *elements into* $\frac{\Pi N}{k}$ *groups, which are* G *and its* $\frac{\Pi N}{k} - 1$ *derived groups.*

We easily demonstrate the known theorem. *The derivate of* G *by* P *is the derivate of* G *by* PA_m, A_m *being any substitution of* G.

Cauchy has also proved in the Memoir above quoted that the substitutions

$$AP \text{ and } PAP^{-1}$$

are *similar substitutions*; that is, that *they differ neither in the number nor in the orders of their circular factors.*

We shall say that the model groups

$$G \text{ and } PGP^{-1}$$

are *equivalent groups,* when they are not the *same group.* All that precedes is well known.

8. The derived groups of G

$$PG, QG, RG, \cdot\cdot$$

are derangements by P, Q, R, $\cdot\cdot$ of the equivalent or identical model groups

$$PGP^{-1}, \; QGQ^{-1}, \; RGR^{-1} \cdot\cdot$$

Let us suppose that one of them is identical with G; or that

$$G = PGP^{-1},$$

then $\qquad GP = PG,$

and consequently $\qquad GP^2 = PGP = P^2G,$

$$GP^c = P^cG,$$
$$G = P^cGP^{-c}.$$

Let $P^m = 1$, we have

$$G(1 + P + P^2 + \cdot\cdot + P^{m-1}) = (1 + P + P^2 + \cdot\cdot + P^{m-1})G.$$

or $$Gg = gG = G',$$

g being the group of the powers of P; and G' is a model group of the km^{th} order; for

$$A_m P^a A_n P^b = A_m P^a P^b A_p = A_m P^c A_p = A_m A_q P^c = A_r P^c,$$

which is the test of a group by the definition, (2).

We shall call the derangements GP, GQ, GR $\cdot\cdot$ of G, which are also the derivates PG, QG, RG$\cdot\cdot$of G, the *derived derangements* of G. And we have the known theorem.

If PG$=$GP *be a derived derangement of* G, G *being of the order* k, *and* P *being a substitution of the* m^{th} *order, there is a group of the* km^{th} *order composed of* G *and its derived derangements by the powers of* P.

It is also known that *if* PG, RG, QG$\cdot\cdot$ *be derived derangements of* G, *the group* G *with its derangemeuts by the powers and by the products of the powers of* PQR$\cdot\cdot$ *forms a model group of substitutions.*

9. Let us suppose that of the equivalent or identical groups above mentioned

$$PGP^{-1} \text{ and } QGQ^{-1}$$

are identical, and different from G.

From

$$PGP^{-1} = QGQ^{-1}$$

it follows that

$$Q^{-1}PGP^{-1}Q = G.$$

Let $$Q^{-1}P = \theta,$$

whence $$P = Q\theta,$$

$$1 = P^{-1}Q\theta,$$

$$\theta^{-1} = P^{-1}Q,$$

which gives by what precedes

$$\theta G\theta^{-1} = G,$$

by which we see that for every group among those under consideration which is identical with PGP^{-1}, there is one identical with G.

Suppose now that

$$PGP^{-1}=G,$$
$$QGQ^{-1}=G',$$

G′ being a different group from G: we have

$$QGQ^{-1}=QPGP^{-1}Q^{-1}.$$

Let us put

$$QP=\theta;$$

whence comes

$$Q=\theta P^{-1},$$

and $$1=\theta P^{-1}Q^{-1},$$

and $$\theta^{-1}=P^{-1}Q^{-1},$$

whence $$G'=QGQ^{-1}=\theta G\theta^{-1}.$$

This proves that for every group among those considered, which is identical with G, there is one identical with G′, G′ being any group equivalent to G.

We have then this theorem—

THEOREM A. *The group* G *of the order k made with* N *elements and its* $\frac{\Pi N}{k}-1$ *derived groups are* $\frac{\Pi N}{k}$ *derangements of* G *and of its equivalents* G_1, G_2, G_3. . . . *And there are among these derived groups neither fewer nor more derangements of* G *than of any equivalent* G_m *of* G.

COR. If the number of groups equivalent to G is $\frac{\Pi N}{k}$, including G, G has no derived derangement, and every derangement of G is a group of permutations different from every derivate of G.

The same is true of the derived groups and derangements of every equivalent to G.

The number of the equivalent groups and of their derangements is $\left(\frac{\Pi N}{k}\right)^2$, which are a system of groups of permutations identical with the system of the $\left(\frac{\Pi N}{k}\right)^2$

derangements of the same equivalents, including the equivalents; for every derivate of G_1 is a derangement of G_2 equivalent to G_1.

If the number of groups equivalent to G is less than that of its derived groups, we know that G has derived derangements. If M be the number of groups equivalent to G, including G, $\frac{\Pi N}{kM}$ is the number of derived derangements of G or of any equivalent to G (including G and the equivalent); and each of the equivalent groups forms with its derived derangements a modular group of $\frac{\Pi N}{M}$ substitutions.

Def. A *modular group* consists of a model G and certain derived derangements of G. A *non-modular group* is a model group which cannot be written as a model with its derived derangements.

§ 3.

Model groups of the order k, made with N *elements, which are the powers of a substitution of the k^{th} order.*

10. Let

$$N = Aa + Bb + Cc + \cdot\cdot + Jj$$

be any partition of N, such that

$$A > B,\ B > C,\ \&c.,\ J > 0,$$

$abc \cdot\cdot j$ being any numbers; and let k be the least common multiple of $ABC \cdot\cdot J$.

We are about to construct a group of the powers of a substitution having a circular factors of the order A (of A elements), b of the order B, &c.

The elements of the first factor of the order A may be selected among the N elements $123 \cdot\cdot N$ in

$$\frac{\Pi N}{\Pi A\ \Pi(N-A)}$$

different ways.

The elements of the second factor of the order A can be chosen among N – A in

$$\frac{\Pi(\mathrm{N}-\mathrm{A})}{\Pi\mathrm{A}\cdot\Pi(\mathrm{N}-2\mathrm{A})}$$

different ways. If $a=2$, we have

$$\frac{1}{1\cdot 2}\cdot\frac{\Pi\mathrm{N}}{(\pi\mathrm{A})^2\,\pi(\mathrm{N}-2\mathrm{A})}$$

different ways of choosing the 2A elements, where we divide by 2, because either of the two circular factors may be considered as the first chosen.

In like manner there are

$$\frac{1}{\Pi a}\cdot\frac{\Pi\mathrm{N}}{(\pi\mathrm{A})^a\,\Pi(\mathrm{N}-\mathrm{A}a)}$$

ways to select the a circular factors of the order A.

In the same way there are

$$\frac{1}{\Pi b}\,\frac{\Pi(\mathrm{N}-\mathrm{A}a)}{(\Pi\mathrm{B})^b\,(\mathrm{N}-\mathrm{A}a-\mathrm{B}b)}$$

ways to select the b factors of the order B from N – Aa elements, and

$$\frac{1}{\Pi c}\,\frac{\Pi(\mathrm{N}-\mathrm{A}a-\mathrm{B}b)}{(\Pi\mathrm{C})^c\,\Pi(\mathrm{N}-\mathrm{A}a-\mathrm{B}b-\mathrm{C}c)}$$

ways to select from the remainder the c circular factors of the order C.

Consequently there are

$$\frac{1}{\Pi a\Pi b\Pi c\cdot\cdot\Pi j}\,\frac{\Pi\mathrm{N}}{(\Pi\mathrm{A})^a(\Pi\mathrm{B})^b(\Pi\mathrm{C})^c\cdot\cdot(\Pi\mathrm{J})^j}=\mathrm{U}$$

different ways to choose the circular factors.

We can write each of the circles of A elements in a vertical row headed by its least element in $\Pi(\mathrm{A}-1)$ ways; whence there are

$$(\Pi(\mathrm{A}-1))^a\,(\Pi(\mathrm{B}-1))^b\,(\Pi(\mathrm{C}-1))^c\cdot\cdot(\Pi(\mathrm{J}-1))^j=\mathrm{V}$$

ways of writing the circles in vertical rows under unity (Art. 4). Wherefore we have thus

$$\mathrm{UV}=\frac{\Pi\mathrm{N}}{\Pi a\,\Pi b\cdot\cdot\Pi j\;\mathrm{A}^a\mathrm{B}^b\mathrm{C}^c\cdot\cdot\mathrm{J}^j}$$

skeleton groups, each of which is to be completed by writing under every element of unity repeated cyclical permutations of the factor of which it forms a part, and continuing the horizontal lines so constructed until the *next line* is unity.

11. It is easy to see that the number of horizontal lines will be k, the least common multiple of $ABC\cdot\cdot J$.

Every group so formed is composed of k successive powers of a substitution of the order k (Art. 4). But the groups will not be all different. It is well known, and easily proved, that any group of k powers of a substitution

$$G = 1 + P + P^2 + P^3 + \cdot\cdot + P^{k-1}$$

can be written in this form in R_k ways, R_k being the number of integers, unity included, which are less than k and prime to it. For there are R_k different substitutions in G similar to P, any one of which, P_1, gives

$$G = 1 + P_1 + P_1^2 + P_1^3 + \cdot\cdot + P_1^{k-1}.$$

Wherefore every group of the UV constructed has been formed R_k times, and we have the exact number of equivalent groups formed on this partition of N by dividing UV by R_k.

12. We have proved the theorems following.

THEOREM B. *The number of equivalent groups of the order* N *which are powers of a substitution of the order* N *made with* N *elements, is* $\dfrac{\Pi(N-1)}{R_N}$, *where* R_N *is the number of integers, unity included, which are less than* N *and prime to it.*

THEOREM C. *If any partition of* N *be*

$$N = Aa + Bb + Cc + \cdot\cdot + Jj,$$
$$A > B,\ B > C \cdot\cdot J > 0,$$

$abc\cdot\cdot j$ *being any numbers, and* k *being the least common multiple of* $ABC\cdot\cdot J$, *the number of equivalent groups, which are powers of a substitution having* a *circular factors of the order* A, b *of the order* B, *&c., is*

$$W = \frac{\Pi N}{R_k \, \Pi a \, \Pi b \, \Pi c \cdot \cdot \Pi j \;\; A^a \, B^b \, C^c \cdot \cdot J^j},$$

where R_k *is the number of integers, unity included, which are less than k and prime to it.*

13. Any two G G′ of the W groups are equivalent, that is, we have

$$G' = \Theta G \Theta^{-1}.$$

Let

$$m_1 m_2 m_3 \cdot \cdot m_m$$

be the m circular factors of G of the order M, and let

$$m'_1 \, m'_2 \, m'_3 \cdot \cdot m'_m$$

be the m circular factors of G′ of the same order. We have

$$\Theta = \frac{a'_1 \, a'_2 \cdot \cdot a'_a \;\; b'_1 \, b'_2 \cdot \cdot b'_b \;\; c'_1 \, c'_2 \cdot \cdot c'_c \cdot \cdot}{a_1 \, a_2 \cdot \cdot a_a \;\; b_1 \, b_2 \cdot \cdot b_b \;\; c_1 \, c_2 \cdot \cdot c_c \cdot \cdot}.$$

For example, let

$$N = 8 = 6 \cdot 1 + 2 \cdot 1 = Aa + Bb.$$

Two of the W groups are

12345678	12345678
36487215	86714532
42851637	25381476
86573241	64728135
52714683	51362874
76138254	48756231

$$\Theta = \frac{a'_1 \, b'_1}{a_1 \, b_1} = \frac{18265437}{13485726} = 13825746,$$

$$G' = \Theta G \Theta^{-1} = 13825746 \;\; G 14275863.$$

§ 4.

Modular groups formed with N *elements of an order superior to* N, *which contain the powers of a substitution of the* N^{th} *order.*

14. We have proved, theorem B(12), that there are $\frac{\Pi(N-1)}{R_N}$ equivalent groups of the powers of a substitution of the N^{th} order made with N elements.

The number of derived groups of each of these is $\Pi(\mathrm{N}-1)$; wherefore each group has $\mathrm{R}_N - 1$ derived derangements, which complete with it a modular group of NR_N substitutions (Cor. theorem A).

We have then the following theorem —

THEOREM D. *With* N *elements we can form* $\dfrac{\Pi(\mathrm{N}-1)}{\mathrm{R}_N}$ *equivalent groups each of* NR_N *substitutions, among which are the powers of a substitution of the* N^{th} *order.*

The simplest of these groups is

$$\begin{array}{lll} 1234\cdot\cdot & \mathrm{N}-1 & \mathrm{N} \\ 2345\cdot\cdot & \mathrm{N} & 1 \\ 3456\cdot\cdot & 1 & 2 \\ 4567\cdot\cdot & 2 & 3 \\ :\,:\,:\,: & : & : \end{array}$$

which may be written

$$\mathrm{G}=\mathrm{S}\left(\frac{i+c}{i}\right) \text{ or } \mathrm{G}=\mathrm{S}(i+c) \pmod{\mathrm{N}},$$

where i is any element of unity and c may have any of the values $012\cdot\cdot\mathrm{N}-1$. For example,

$$56781234=\frac{i+4}{i}=i+4 \pmod{8}.$$

Let $p>1$ be any integer $<\mathrm{N}$ and prime to N. The substitution

$$\mathrm{P}=pi+c \pmod{\mathrm{N}}$$

is made by putting for i the residue of $pi+c$ according to modulus N.

We form the product

$$\mathrm{P'P}=(p'i+c')\,(pi+c) \pmod{\mathrm{N}}$$

where p' and p are both $<\mathrm{N}$ and prime to it, by writing in P′ for $p'i+c'$

$$p'(pi+c)+c' \pmod{\mathrm{N}}.$$

This product is

$$\begin{aligned}(p'i+c')(pi+c)&=p'(pi+c)+c'\\ &=p'pi+p'c+c'=p''i+c'' \pmod{\mathrm{N}},\end{aligned}$$

where p'' also is $<\mathrm{N}$ and prime to it, and c'' is $<\mathrm{N}$.

This proves that the $R_N N$ substitutions of the form $(p_{\prime\prime} i+c)$ (mod. N) $(p_{\prime\prime} \overline{\gtrless} 1)$ form a model group.

The $R_N - 1$ derived groups given by $R_N - 1$ values of $p > 1$,

$$PG = pi S(i+c),$$

are the $R_N - 1$ derangements

$$GP = S(i+c)\,pi;$$

for $pi(i+c) = p(i+c) = pi + pc = pi + c'$ and $(i+c)pi = pi + c$; that is, every substitution in PG is a substitution in GP.

The group of $R_N N$ substitutions has none of the order N except those of $G = S(i+c)$. For if any of the derived derangements added to G contained Q of the order N, there would be the $N-1$ derived derangements

$$QG\ \ Q^2G \cdot\cdot Q^{N-1}G_1 \ \text{(Art. 8)};$$

but we know that G has not more than $N-2$ derived derangements, for $R_N - 1 \not> N-2$.

We shall presently show how the groups equivalent to the simple modular group

$$G' = S(pi+c),$$

where $p \overline{\gtrless} 1$ has R_N values and $c < N$ has N values, may be constructed, each of $N \cdot R_N$ substitutions.

16. Let q be any integer less than N and prime to it, and let it be a prime root of the congruence

$$x^r - 1 \equiv 0 \ \text{(mod. N)},$$

where $q \overline{\gtrless} R_N$, that is to say, such a root that we cannot have

$$q^{r_1} \equiv 1 \ \text{(mod. N)},$$

r_1 being less than r.

We know that q^e is also a root of the congruence

$$x^r - 1 \equiv 0 \ \text{(mod. N)}.$$

We have proved that the products

$$qiS(i+c),\ q^2iS(i+c) \cdot\cdot q^{r-1}iS(i+c)$$

are derived derangements of the group

$$G = S(i+c),$$

and they consequently form with G a group of Nr substitutions of the form

$$q^a i + c\,;$$

and in fact we see that

$$(q^a i + c)\cdot(q^b i + c') = q^a(q^b i + c') + c$$
$$= q^e i + q^a c' + c = q^e i + c'' \ (\text{mod. N}),$$

where $e \overline{\gtrless} r$, and $c'' \overline{\gtrless} \text{N} - 1$.

Let

$$\text{G} + \text{QG} + \text{Q}^2\text{G} + \cdots + \text{Q}^r\text{G} = \text{H}$$

be this group of Nr substitutions. H will be a portion of the group G′ above found by adding to G all its derived derangements. H is a factor of the group G′.

If there be m roots of the congruence

$$x^r - 1 \equiv 0 \ (\text{mod. N}),$$

of which no one is comprised among the powers of another, according to the modulus N, they will give m different groups each of the order Nr, and each a factor of G′; but if two of these roots have a common power according to modulus N_1, the groups which they determine of the Nr^{th} order will have a common portion.

17. We have shown that there are $\dfrac{\Pi(\text{N}-1}{\text{R}_N} - 1$ other groups equivalent to G′, and each of these will have a factor equivalent to H. We have then the following

THEOREM E. *If there be among the* $\text{R}_N - 1$ *integers,* > 1, *inferior to* N *and prime to it, a prime root of the congruence*

$$x^r - 1 \equiv 0 \ (\text{mod. N}),$$

where $r \overline{\gtrless} \text{R}_N$, *we can form with* N *elements* $\dfrac{\Pi(\text{N}-1)}{\text{R}_N}$ *equivalent groups each of* Nr *substitutions, containing the powers of a substitution of the* Nth *order. And if there are m prime roots of this congruence of which none is comprised among the powers of another, according to the modulus, we can form* $\dfrac{m\Pi(\text{N}-1)}{\text{R}_N}$ *different groups each of the order* Nr, *forming sets each of* $\dfrac{\Pi(\text{N}-1)}{\text{R}_N}$ *equivalent modular groups.*

18. It may be useful to give examples on the theorems D and E. Take $N=5$. We should have by theorem D $\frac{\Pi 4}{4}=6$ equivalents groups of 20 substitutions. The simplest of these is made by adding to $G=S(i+c)$ its derivates by $2i$, $3i$ and $4i$. This gives the group G'

12345	24135	31425	43215
23451	41352	14253	32154
34512	13524	42531	21543
45123	35241	25314	15432
51234	52413	53142	54321

which is found in the Memoir of Cauchy, above quoted, and which has been given by Betti, and first, as I believe, by Galois. I cannot find that any of the equivalent groups has been formed, or that their enumeration has been distinctly affirmed by previous writers.

We see that the three added groups are derived derangements, by writing them thus:

24135	31425	43215
35241	42531	54321
41352	53142	15432
52413	14253	21543
13524	25314	32154

The equivalent groups of the twentieth order are

$$12453\ G'12534=G'_1$$
$$12534\ G'12453=G'_2$$
$$12354\ G'12354=G'_3$$
$$12435\ G'12435=G'_4$$
$$12543\ G'12543=G'_5$$

all of the form $Q\,G'Q^{-1}$.

In order to determine Q, we compare with G any one g of its equivalents, thus:

$g=12345$	$G=12345$
24531	23451
43152	34512

35214	45123
51423	51234

The circular factor 12435 of g is obtained by multiplying the circular factor of G by

$$\frac{12435}{12345}=12435=\mathrm{Q}\,.$$

The derived of G by Q is

$$\begin{aligned}\mathrm{QG}=\,&12435\\ &24351\\ &43512\\ &35124\\ &51243\end{aligned}$$

and the derangement of QG by $\mathrm{Q}^{-1}=12435$ is

$$\begin{aligned}g=\,&12345\\ &24531\\ &43152\\ &35214\\ &51423\end{aligned}$$

wherefore $g=12435\,\mathrm{G}\,12435=\mathrm{QGQ}^{-1}$.

All that we have to do is to write, for each one (c) of the R_N circular factors of G and its derived derangements, $12435(c)$, and for the first permutation A of each group of five, $12435\,\mathrm{A}(12435)^{-1}=12435\,\mathrm{A}12435$, thus:

12345	23451	41235	34125
2	3	1	4
4	1	3	2
3	4	2	1
5	5	5	5

The equivalent group $12435\,\mathrm{G}'\,12435$ is completed thus:

12345	23415	41235	$34125=\mathrm{G}'_4$
24531	31542	13524	42513
43152	14253	32451	21354
35214	45321	25143	15432
51423	52134	54312	53241.

The others are

12345	25314	41352	54321 $= G'_1$
24153	51243	15423	42513
45231	14532	52134	21435
53412	43125	23541	13254
31524	32451	34215	35142

12345	23541	51243	35142 $= G'_2$
25413	31425	13452	52431
53124	15234	32514	21354
34251	54312	24135	14523
41532	42153	45321	43215

12345	24351	51324	45312 $= G'_5$
25134	41235	14532	52431
54213	15423	42153	21543
43521	53142	23415	13254
31452	32514	35241	34125

12345	25143	31542	53241 $= G'_3$
23514	51324	15234	32154
35421	13452	52413	21435
54132	34215	24351	14523
41253	42531	43125	45312.

Each of these consists of a group of powers of a substitution of the fifth order and of its derived derangements.

19. Whatever N may be we have always

$$(N-1)^2-1 \equiv 0 \pmod{N}.$$

If then we add to $G=S(i+c)$ the derived

$$Q'G=(N-1)i\cdot G=-iG \pmod{N},$$

we have (theorem E) a group of 2N substitutions.

The first and last of the four groups of five in G′(18) make such a group of ten.

All the substitutions of the derived Q′G have the form

$$q=-(i+c)=-i-c \pmod{N},$$

and

$$q^2=-(i+c)\cdot-(i+c)=i+c-c=i=(1);$$

that is, all these substitutions (Q′G) are square roots of unity.

The last derived group of $G'_1 G'_2 G'_3 G'_4 G'_5$, like the last of G′, is composed of square roots of unity.

We have then the theorem following—

THEOREM F. *With* N *elements we can form* $\frac{\Pi(N-1)}{R_N}$ *equivalent groups each of* 2N *substitutions, which are* N *powers of one substitution and* N *square roots of unity.*

Take N = 7; we have three roots of the congruence

$$x^3 - 1 \equiv 0 \pmod{7},$$

viz., 1, 2 and 4. We have the group (theorem E)

$$S(i+c) + 2iS(i+c) + 4iS(i+c)$$

of the twenty-first order, and this has 119 equivalents.

If N = 8, we have four roots of

$$x^2 - 1 \equiv 0 \pmod{8},$$

viz., 1, 3, 5, 7, of which 3, 5 and 7 are prime roots. We can therefore form three groups (theorem E) each of sixteen substitutions, all containing the same group of eight powers of a substitution. We have only to add, to (G) the eight cyclical permutations of 12345678, the derived groups

$$3iG = 36147258G,$$
$$5iG = 52741638G,$$
$$7iG = 76543218G.$$

The last of these is composed of eight square roots of unity. There are $\frac{3 \cdot \Pi 7}{R_8} = 3 \cdot 7 \cdot 6 \cdot 5 \cdot 3 \cdot 2$ different groups of sixteen, each comprising eight powers of a substitution; and in the group $G + 5iG$ there are all the powers of two different substitutions of the eighth order.

§ 5.

Groups of N(N − 1) (N − 2) *substitutions made with* N *letters, which comprise substitutions of the* N^{th} *order, where* N − 1 *is a prime number.*

20. Let $g_0 = S(pi+c)$ (mod. (N − 1)) be the group of $(N-1)R_{N-1}$ substitutions made with N − 1 elements of theorem D and of Art. (15), when N − 1 is prime, in which case

$$(N-1)R_{N-1} = (N-1)(N-2).$$

Let the final element N be added to every one of the $(N-1)(N-2)$ substitutions of g_0. This gives a group made with N elements of the order $(N-1)(N-2)$,

$$g \equiv S(pi+c) \ (\text{mod. } (N-1)) + \frac{N}{N}$$

where i is any number $<N$, p is any number >0 and $<N-1$, and c is any number $<N$, $\overline{\gtrless} 0$.

The substitutions of g are formed by writing $pi+c$ for i, when $i<N$, and N for $i=N$.

This

$$g = 1 + P_1 + P_2 + P_3 + \cdots,$$

with the N − 1 derived groups

$$\Psi_1 g, \ \Psi_2 g, \ \Psi_3 g \cdots \Psi_{N-1} g,$$

completes a group of $N(N-1)(N-2)$ substitutions; if

$$\Psi_1 \equiv \frac{\{\beta^{-(1+x)} + 1\}_{\text{mod. }(N-1)}}{\{\beta^x + 1\}_{\text{mod. }(N-1)}} + \frac{1}{N} + \frac{N}{1},$$

and if, for $r > 1$,

$$\Psi_r \equiv \frac{\{\beta^{-(1+x)}(1-r) + r\}_{\text{mod. }(N-1)}}{\{\beta^x(1-r) + r\}_{\text{mod. }(N-1)}} + \frac{N}{r} + \frac{r}{N};$$

where β is any primitive root of (N − 1), and where the numerators and denominators of the first fractions are residues estimated to modulus (N − 1). Every one of the elements $<N$ can be represented by a finite value of x in the first denominator of Ψ_r except the element r, and the rest of the expression gives the substitutions to be made for r and for N.

If we admit an infinite value of x, the value r is not excluded from those represented in the terms of the first fraction in Ψ_r, and the substitution retains its definite character.

We shall suppose then that x has any value finite or infinite.

It is necessary that we prove, first that the $\mathrm{N}-1$ derived groups

$$\Psi_1 g\ \Psi_2 g \cdot\cdot \Psi_{N-1} g$$

are all different, and secondly that they form with g a group of substitutions.

If they be not all different, we shall have either

$$\Psi_1 g = \Psi_m g,\ (m > 1),$$

or

$$\Psi_r g = \Psi_m g;\ (r > 1,\ \lesseqgtr m).$$

In the former case Ψ_m will be among the substitutions of $\Psi_1 g$; that is,

$$\Psi_m = \frac{\{\beta^{-(1+x)}(1-m)+m\}_{\text{mod. }(N-1)}}{\{\beta^x(1-m)+m\}_{\text{mod. }(N-1)}} + \frac{\mathrm{N}}{m} + \frac{m}{\mathrm{N}}$$

$$= \left\{\frac{\{\beta^{-(1+x)}+1\}_{\text{mod. }(N-1)}}{\{\beta^x+1\}_{\text{mod. }(N-1)}} + \frac{1}{\mathrm{N}} + \frac{\mathrm{N}}{1}\right\} \left(\frac{pi+c}{i} + \frac{\mathrm{N}}{\mathrm{N}}\right).$$

Let

$$i \equiv \beta^z(1-m)+m,\ \text{mod. } (\mathrm{N}-1),$$

and let

$$p(\beta^z(1-m)+m)+c \equiv \beta^y + 1,$$

which can be satisfied by a value of y, whatever z may be.

The effect of the substitution Ψ_m (in the right member) is to change i into

$$\beta^{-(1+y)}+1,$$

and the effect of the same Ψ_m (in the left member) is to change i into

$$\beta^{-(1+z)}(1-m)+m.$$

We have then the two equations

$$p(\beta^z(1-m)+m)+c \equiv \beta^y+1$$
$$\beta^{-(1+y)}+1 \equiv \beta^{-(1+z)}(1-m)+m;$$

whence comes

$$\{p(\beta^z(1-m)+m)+c\}\{\beta^{-z}(1-m)+2m-2\} = 1,$$

which must be true, if Ψ_m is among the substitutions of $\Psi_1 g$, for every value of z. This is impossible, unless the coefficient of β^z is zero; that is, unless

$$2p(1-m)(m-1) \equiv 0, \text{ mod. } (\mathrm{N}-1),$$

for a value of $m > 1$; which is absurd.

Wherefore Ψ_m is not in $\Psi_1 g$, and $\Psi_m g$ is different from $\Psi_1 g$.

Neither is Ψ_m among the substitutions of $\Psi_r g$. For if

$$\Psi_m = \frac{\{\beta^{-(1+x)}(1-m)+m\}_{\text{mod. }(N-1)}}{\{\beta^x(1-m)+m\}_{\text{mod. }(N-1)}} + \frac{\mathrm{N}}{m} + \frac{m}{\mathrm{N}}$$

$$= \left\{\frac{\{\beta^{-(1+x)}(1-r)+r\}_{\text{mod. }(N-1)}}{\{\beta^x(1-r)+r\}_{\text{mod. }(N-1)}} + \frac{\mathrm{N}}{r} + \frac{r}{\mathrm{N}}\right\} \left(\frac{pi+c}{i} + \frac{\mathrm{N}}{\mathrm{N}}\right),$$

let

$$i \equiv \beta^z(1-m)+m,$$

and let

$$p(\beta^z(1-m)+m)+c \equiv \beta^y(1-r)+r.$$

The effect of the substitution Ψ_m is to change i into

$$\beta^{-(1+y)}(1-r)+r$$

in the right member, and i into

$$\beta^{-(1+z)}(1-m)+m$$

in the left member. We deduce from

$$\beta^{-(1+y)}(1-r)+r \equiv \beta^{-(1+z)}(1-m)+m,$$

and from the last written equation,

$$2p(1-m)(m-r) \equiv 0, \text{ mod. } (\mathrm{N}-1),$$

for the coefficient of β^z. This requires either $m=1$, or $m=r$, both contrary to hypothesis.

It is thus demonstrated that the $\mathrm{N}-1$ derived groups are all different.

It is requisite in the next place to prove that they form with g a group of substitutions. This is established if we can demonstrate the two propositions

$$\Psi_q \Psi_r \equiv \Psi_s \mathrm{P}_n$$

$$\Psi_q \mathrm{P}_m \equiv \mathrm{P}_n \Psi_k$$

for all values of q, r and m; where P_u, P_m and P_n are substitutions of the form

$$\frac{(pi+c)}{i} + \frac{\mathrm{N}}{\mathrm{N}},$$

in which $p > 0 < \mathrm{N}-1$, and $c < \mathrm{N}$, $\overline{\gtrless} 0$;

for we shall have, in consequence of these,

$$\Psi_q \mathrm{P}_a \Psi_f \mathrm{P}_e \equiv \Psi_q \mathrm{P}_a \mathrm{P}_d \Psi_h \equiv \Psi_q \mathrm{P}_m \Psi_h \equiv \Psi_q \Psi_r \mathrm{P}_c$$
$$\equiv \Psi_s \mathrm{P}_u \mathrm{P}_c \equiv \Psi_s \mathrm{P}_k,$$

or the product of any two substitutions is a substitution of the group.

21. We have first to show that (the modulus being $\mathrm{N}-1$),

$$\Psi_q \Psi_r \equiv \Psi_s \mathrm{P}_u,$$

where

$$\mathrm{P}_u = \frac{pi+c}{i} + \frac{\mathrm{N}}{\mathrm{N}};$$

that is, that

$$\left\{\frac{\beta^{-(z+1)}(1-q)+q}{\beta^x(1-q)+q} + \frac{q}{\mathrm{N}} + \frac{\mathrm{N}}{q}\right\}\left\{\frac{\beta^{-(x+1)}(1-r)+r}{\beta^x(1-r)+r} + \frac{r}{\mathrm{N}} + \frac{\mathrm{N}}{r}\right\}$$
$$\equiv \left\{\frac{\beta^{-(x+1)}(1-s)+s}{\beta^x(1-s)+s} + \frac{s}{\mathrm{N}} + \frac{\mathrm{N}}{s}\right\}\left\{\frac{(pi+c)}{i} + \frac{\mathrm{N}}{\mathrm{N}}\right\}.$$

Let

$$i \equiv \beta^x(1-r)+r \quad (\text{mod. } (\mathrm{N}-1)),$$

and let

$$\beta^{-(x+1)}(1-r)+r \equiv \beta^z(1-q)+q, \quad (\text{mod. } (\mathrm{N}-1)),$$

which can be satisfied by a value of z, whatever x may be.

The result of the substitution $\Psi_q\Psi_r$ is to change i into

$$\beta^{-(z+1)}(1-q)+q.$$

Let

$$p(\beta^x(1-r)+r)+c \equiv \beta^v(1-s)+s,$$

which can be satisfied by a value of v, whatever x may be.

The result of the substitution $\Psi_s \mathrm{P}_u$ is to change i into

$$\beta^{-(v+1)}(1-s)+s.$$

We have then the equations

$$\beta^{-(x+1)}(1-r)+r \equiv \beta^z(1-q)+q, \qquad (a)$$
$$p(\beta^x(1-r)+r)+c \equiv \beta^v(1-s)+s, \qquad (b)$$
$$\beta^{-(z+1)}(1-q)+q \equiv \beta^{-(v+1)}(1-s)+s. \quad (c)$$

From $(a)(c)$ we obtain

$$\{\beta^{-(x+1)}(1-r)+r-q\}\{\beta^{-v}(1-s)+\beta(s-q)\} \equiv (1-q)^2,$$

and from this and (b) comes

$$\{(1-q)^2-(\beta^{-(x+1)}(1-r)+r-q)\,\beta(s-q)\}\,\{p(\beta^x(1-r)+r)+c-s\}$$
$$\equiv (1-s)^2\{\beta^{-(x+1)}(1-r)+r-s\},$$

which is to be true for all values of x. This requires that the coefficients of the three powers of β^x in this equation written $U=0$ shall be zeros. And by these three conditions we can determine p, c and s in terms of q and r.

Wherefore it is evident that

$$\Psi_q \Psi_r \equiv \Psi_s \mathrm{P}_u,$$

when $r>1$; and if $\Psi_r=\Psi_1$, the same thing is demonstrated by beginning with $i\equiv\beta^x+1$; and if $\Psi_q=\Psi_1$, we still obtain three equations $(a)(b)(c)$, by which we can determine p, c and s.

22. It is necessary, in the next place, to demonstrate that

$$\Psi_q \mathrm{P}_m \equiv \mathrm{P}_n \Psi_k;$$

that is, that

$$\left\{\frac{\beta^{-(x+1)}((1-q)+q)}{\beta^x(1-q)+q}+\frac{q}{\mathrm{N}}+\frac{\mathrm{N}}{q}\right\}\left\{\frac{p_1 i+c_1}{i}+\frac{\mathrm{N}}{\mathrm{N}}\right\}$$
$$\equiv\left\{\frac{pi+c}{i}+\frac{\mathrm{N}}{\mathrm{N}}\right\}\left\{\frac{\beta^{-(x+1)}(1-k)+k}{\beta^x(1-k)+k}+\frac{k}{\mathrm{N}}+\frac{\mathrm{N}}{k}\right\},$$

and that p_1, c_1 and q are given in terms of p, c and k.

Let $i\equiv\beta^x(1-k)+k$.

The effect of the substitution $\mathrm{P}_n\Psi_k$ is to change i into

$$\beta^{-(x+1)}(1-k)+k,$$

and then to change this into

$$p(\beta^{-(x+1)}(1-k)+k)+c.$$

Let

$$p_1(\beta^x(1-k)+k)+c_1\equiv\beta^z(1-q)+q, \qquad (a)$$

which can be satisfied by a value of z, whatever be x. The effect of the substitution $\Psi_q\mathrm{P}_m$ is to change i into

$$\beta^{-(z+1)}(1-q)+q;$$

so that

$$p(\beta^{-(x+1)}(1-k)+k)+c\equiv\beta^{-(z+1)}(1-q)+q. \qquad (b)$$

The equations (a) (b) give

$$\{p_1(\beta^x(1-k)+k)+c_1-q\}\{\beta p(\beta^{-(x1+)}(1-k)+k)+\beta(c-q)\}$$
$$\equiv(1-q)^2,$$

which is to be true for all values of x. This requires that the coefficients of the three powers β^x should be zeros,

which three conditions determine p_1, c_1 and q in terms of p, c and k.

The same process will determine p_1, c_1 and q in terms of p and c, if $\Psi_k = \Psi_1$, and if a beginning is made with

$$i = \beta^x + 1.$$

Wherefore it is demonstrated that

$$\Psi_q \mathrm{P}_m \equiv \mathrm{P}_n \Psi_k,$$

and that

$$\mathrm{G} = g + \Psi_1 g + \Psi_2 g + \cdots + \Psi_{N-1} g$$

is a model group of

$$\mathrm{N}(\mathrm{N}-1)(\mathrm{N}-2) \text{ substitutions.}$$

23. We can modify the form of the substitutions $\Psi_1 \Psi_2 \cdots$ thus. In

$$\Psi_1 = \frac{\beta^{-(x+1)} + 1}{\beta^x + 1} + \frac{1}{\mathrm{N}} + \frac{\mathrm{N}}{1},$$

let

$$i = \beta^x + 1.$$

We have

$$\beta^{-(x+1)} + 1 = (i-1)^{-1}\beta^{-1} + 1 = \beta^{N-3}(i-1)^{N-3} + 1\,;$$

wherefore

$$\Psi_1 = \frac{\beta^{N-3}(i-1)^{N-3} + 1}{i}{}_{(\text{mod. } (N-1))} + \frac{1}{\mathrm{N}} + \frac{\mathrm{N}}{1},$$

In

$$\Psi_r = \frac{\beta^{-(x+1)}(1-r) + r}{\beta^x(1-r) + r} + \frac{r}{\mathrm{N}} + \frac{\mathrm{N}}{r},$$

let

$$i = \beta^x(1-r) + r,$$

whence

$$(1-r)(i-r)^{-1} = \beta^{-x}$$

and

$$\beta^{-(x+1)}(1-r) + r = (1-r)^2(\beta i - \beta r)^{-1} + r$$

and

$$\Psi_r = \frac{(1-r)^2\beta^{N-3}(i-r)^{N-3} + r}{i}{}_{(\text{mod. } (N-1))} + \frac{r}{\mathrm{N}} + \frac{\mathrm{N}}{r}\,.$$

The group of $\mathrm{N}(\mathrm{N}-1)(\mathrm{N}-2)$ substitutions may be written

$$\mathrm{G} = \left\{\mathrm{S}(pi + c) + \frac{\mathrm{N}}{\mathrm{N}}\right\}\{1 + \Psi_1 + \Psi_2 + \Psi_3 + \cdots + \Psi_{N-1}\},$$

where Ψ_1 and Ψ_r $(r > 1)$ are the substitutions above written.

In
$$\left\{S(pi+c)+\frac{N}{N}\right\}$$
i has all values $<N$,
p has all values $<N-1$, >0,
c has all values $<N-1$, $\overline{\overline{>}}0$.

There are, of necessity, $\dfrac{\Pi N}{N(N-1)(N-2)}-1$ groups

$$\theta G\theta^{-1}\ \theta_1 G\theta_1^{-1}\ \theta_2 G\theta_2^{-1}\cdots\ \&c.\ (\text{Art. } 9),$$

of which each is either identical with G or equivalent to it.

Every equivalent group will have, as G has, $(N-2\cdot N-1)$ substitutions, which have N in its natural place; and if N be erased, we shall have a group of $\overline{N-2}\cdot\overline{N-1}$ substitutions made with $N-1$ elements. We have proved (theorem D) (14), that there are $\Pi(N-3)$ equivalent groups of $\overline{N-2}\cdot\overline{N-1}$ substitutions, when $N-1$ is a prime number. It follows that there are $\Pi(N-3)$ equivalent groups of the order $N\,\overline{N-1}\cdot\overline{N-2}$, of which G above constructed is one.

Wherefore we have the theorem following—

Theorem F. *If* $N-1$ *be any prime number, there are* $\Pi(N-3)$ *equivalent groups of the order* $N\cdot\overline{N-1}\ \overline{N-2}$.

It is a property of these groups, that any one of them, or any derived group of any one of them, gives a solution of this problem:

To seat N *persons*, $N-1$ *being any prime number*, $N\cdot(N-1)(N-2)$ *times in* N *chairs, so that no three persons shall twice occupy the same three chairs.*

There are $(\Pi(N-3))^2$ different ways of solving this tactical problem.

§ 6.

Groups of the form $G+RG$, *where* R *is composed of square roots of unity.*

24. Let

$$(p_1p_2p_3\cdots p_a)(q_{a+1}q_{a+2}\cdots q_{a+b})(r_{a+b+1}r_{a+b+2}\cdots)\ \&c.$$

be the circular factors of the orders ABC· ·, of a group G made on the partition of N elements (Art. 12, theorem C),

$$N = Aa + Bb + Cc + \cdots + Jj$$
$$(A > B,\ B > C,\ \&c.),\ (A > 2).$$

Let

$$p_m = a_m\, b_m\, c_m \cdots h_m,\ \text{(A elements)}$$
$$q_m = a'_m\, b'_m\, c'_m \cdots f'_m \cdots \text{(B elements)}$$
$$\vdots$$
$$p_{-m} = a_m\, h_m\, g_m \cdots b_m$$
$$q_{-m} = a'_m\, f'_m\, e'_m \cdots b'_m,$$

and let

$$p^i_m\ p^i_{-m}\ q^i_m\ q^i_{-m} \cdots$$

be the i^{th} cyclical permutations of the factors

$$p_m p_{-m} \cdots q_m,\ q_{-m};\ (p^i_m = p_m^{Ae+i}).$$

The substitution

$$R = \frac{p_{-1}\, p^{\alpha}_{-2}\, p^{\beta}_{-3} \cdots q^{\gamma}_{-(a+1)}\ q^{\delta}_{-(a+2)} \cdots}{p_1\ \ p_2\ \ p_3 \cdots\ q_{a+1}\quad q_{a+2}\ \cdots}$$
$$= \frac{p^n_{-1}\, p^{a+n-1}_{-2}\ p^{a+n-1}_{-3} \cdots q^{\gamma+n-1}_{-a+1}\ q^{\delta+n-1}_{-(+2)} \cdots}{p^n_1\quad p^n_2\qquad p^n_3\ \cdots\ q^n_{a+1}\quad q^n_{a+2}\ \cdots}$$

is a square root of unity; for, let

$$p^n_1 = abcdefg;$$

we have

$$\frac{p^n_{-1}}{p^n_1} = \frac{agfedcb}{abcdefg}$$
$$\frac{p^{n+1}_{-1}}{p^n_1} = \frac{gfedcba}{abcdefg}$$
$$\frac{p^{n+2}_{-1}}{p^n_1} = \frac{fedcbag}{abcdefg}$$

&c.

We see that all the factors in these substitutions are of the second order.

25. Further: RG is a derived derangement of G, of which all the substitutions are square roots of unity. For the vertical circles of RG are those of G read in contrary order; and since, if

$$G = 1 + P + P^2 + P^3 \cdot\cdot$$

$$P^{n-1} = \frac{p_1^n\, p_2^n \cdot\cdot q_{a-1}^n\, q_{a-2}^n \cdot\cdot}{p_1\, p_2 \cdot\cdot q_{a+1}\, q_{a+2} \cdot\cdot},$$

we have

$$RP^{n-1} = \frac{p_{-1}^n\, p_{-2}^{\alpha+n-1}\, p_{-3}^{\beta+n-1} \cdot\cdot q_{-(a+1)}^{\gamma+n-1}\, q_{-(a+2)}^{\delta+n-1} \cdot\cdot}{p_1 \quad p_2 \quad p_3 \quad \cdot\cdot \quad q_{a+1} \quad q_{a+2} \quad \cdot\cdot}$$

which has no factors but of the second order, as we have just proved.

The number of these derivants R, all beginning in the denominator and in the numerator, with the same element 1 of

$$p_1 = 1abc \cdot\cdot$$

is

$$A^{a-1} B^b C^c \cdot\cdot J^j;$$

for every factor

$$p_{-2}\, p_{-3} \cdot\cdot p_{-a}\, q_{-(a+1)} \cdot\cdot q_{-(a+b+1)}$$

may have A different or B different exponents, according as it is a p or q, &c.

Let $M < k$ (theorem C); we have

$$RP^M = \frac{p_{-1}^{M+1}\, p_{-2}^{M+\alpha}\, p_{-3}^{M+\beta} \cdot\cdot q_{-(a+1)}^{\gamma+M}\, q_{-(a+2)}^{\delta+M}}{p_1 \quad p_2 \quad p_3 \quad \cdot\cdot \quad q_{a+1} \quad q_{a+2}},$$

which cannot be identical with R, unless

$$M = Ae = Be = Ce = \cdot\cdot \ \&c.,$$

which is impossible, because k (Art. 12) is greater than M.

Wherefore RG has k different substitutions.

The derived group RG comprises the e substitutions

$$RP^A,\ RP^{2A},\ \cdot\cdot\ RP^{Ae},$$

where

$$e = \frac{k}{A} - 1,$$

which differ from R only in the exponent of

$$q_{-(a+1)}\, q_{-(a+2)} \cdot\cdot$$

in the numerator. There are, therefore, $e + 1$ systems of exponents among those enumerated which give the same derived group,

$$RG = RP^A G = RP^{2A} G = \cdot\cdot = RP^{Ae} G.$$

It follows that the number of different systems of exponents is

$$\frac{1}{e+1}\cdot A^{a-1}B^bC^c\cdot\cdot J^j=\frac{1}{k}\cdot A^aB^bC^c\cdot\cdot J^j.$$

26. We can take, for example,

$$N=6=3\cdot 2=Aa.$$

One of the $W=20$ groups of theorem C (12) is

$$\begin{aligned}G=\,&123456\\&461325\\&354162\,.\end{aligned}$$

We have

$$p_1=143 \qquad p_2=265$$
$$p_{-1}=134 \quad p_{-2}=256\,;$$

and we can employ the three derivants

$$R=\frac{p_{-1}\,p_{-2}}{p_1\ \ p_2}=124365$$

$$R'=\frac{p_{-1}\,p^2_{-2}}{p_1\ \ p_2}=154326$$

$$R''=\frac{p_{-1}\,p^3_{-2}}{p_1\ \ p_2}=164352$$

which give the three groups

123456	123456	123456
461325	461325	461325
354162	354162	354162
124365	154326	164352
351426	361452	321465
463152	423165	453126
H	H_1	H_2.

These are equivalent groups; for we have

$$H_1=164352H164352=QHQ^{-1}$$
$$H_2=154326H154326\,.$$

We may take another of the 20 groups of theorem C.

$$\begin{aligned}G'=\,&123456 \qquad p'_1=135 \qquad p'_2=264\\&365214 \quad\ p'_{-1}=153 \quad p'_{-2}=246\\&541623\end{aligned}$$

$$\mathrm{R'G'} = \begin{matrix}145236\\563412\\321645\end{matrix} = \frac{p'_{-1}\,p^2_{-2}}{p'_1\;p'_2}\mathrm{G'}.$$

This group $\mathrm{H}_3=\mathrm{G'}+\mathrm{R'G'}$ is equivalent with the preceding. We have, for example,

$$\mathrm{H}_3 = 125346\mathrm{H}_1(125346)^{-1},$$

$$=\left(\frac{p'_1\,p'_2}{p_1\,p_2}\right)\ \mathrm{H}_1\left(\frac{p'_1\,p'_2}{p_1\,p_2}\right)^{-1}.$$

In like manner, it may be proved that all the groups are equivalent which we enumerate in the theorem following.

THEOREM G. *Let*

$$\mathrm{N}=\mathrm{A}a+\mathrm{B}b+\cdots+\mathrm{J}j$$

$$\mathrm{A}>2,\ \mathrm{A}>\mathrm{B},\ \mathrm{B}>\mathrm{C}\cdots,$$

and let k be the least common multiple of ABC··J. *We can construct in this partition of* N,

$$\frac{\Pi\mathrm{N}}{\mathrm{R}_k\,k\,\Pi a\,\Pi b\cdots\Pi j}$$

equivalent groups of 2K *substitutions, each of the form* G+RG, *where* G *is one of the groups of k powers* (*of theorem* C), *and* RG *is composed of k square roots of unity.*

§ 7.

Grouped groups of the first class.

27. Let

$$\mathrm{N}=\mathrm{A}a\mathrm{B}b+\cdots+\mathrm{J}j$$

$$\mathrm{A}>\mathrm{B}>\mathrm{C}\cdots>\mathrm{J}$$

be any partition of N.

A substitution P is a principal substitution of a group formed on this partition, when it has the form

$$f\mathrm{P}=\mathrm{A}a+\mathrm{B}b+\cdots+\mathrm{J}j,$$

which means that P has a circular factors of the A^{th}, b of the B^{th} order, &c.; and if the form of every other substitution Q of the group is

$$f\mathrm{Q}=\mathrm{A'}a'+\mathrm{B'}b'+\mathrm{C'}c'+,$$

such that the first of the differences

$$A-A',\ a-a',\ B-B',\ b-b',\ C-C',\ c-c'\ \cdot\cdot,$$

which is not zero, is positive. If they are all zero, Q also is a principal substitution.

We are about to enumerate groups having their principal substitutions defined by this partition of N.

A grouped group has elementary groups of which those of the same form are permuted, sometimes as though they were single unchangeable elements, and more frequently so that they suffer certain interior derangements while they are transposed. For example,

$$\begin{matrix} 1234 \\ 2143 \\ 3412 \\ 4321 \end{matrix}$$

is a grouped group, in which are seen two elementary groups $\begin{matrix} 12 \\ 21 \end{matrix}$ and $\begin{matrix} 34 \\ 43 \end{matrix}$.

Another grouped group is

$$\begin{matrix} 1234 \\ 2143 \\ 3421 \\ 4312\,. \end{matrix}$$

In the first the elementary groups are simply permuted; in the second they are permuted while one suffers at the same time a certain interior derangement.

28. Let G be a group of theorem C (12); and let

$$p_1 p_2 \cdot \cdot p_a$$

be the a circular factors of A^{th} order,

$$q_{a+1}\, q_{a+2} \cdot \cdot q_{a+b}$$

the b of the B^{th} order,

$$r_{a+b+1}\, r_{a+b+2} \cdot \cdot r_{a+b+c}$$

the c of the C^{th} order, and let

$$p_m^i = p_m^{hA+}$$

be the i^{th} cyclical permutation of p_m, &c.

Let g be any model group of l substitutions formed with

$$a+b+c+\cdots+j$$

elements, such that the first a vertical rows of g written in a column shall contain only the first a elements of unity, the b next vertical rows shall contain none but the next b elements, &c.

Let this group be

$$g=1+\Theta+\cdots+\Theta'+\cdots+(\Theta\Theta')+\cdots$$

where

$$\begin{array}{rcl} 1 & = & 1234\cdots a\cdots\quad\cdots \\ \Theta & = & \alpha\beta\gamma\delta\;\cdots\theta\eta\cdots\epsilon\cdots \\ \vdots & & \\ \Theta' & = & \kappa\lambda\mu\nu\;\cdots\rho\cdots\quad\cdots \\ \vdots & & \\ (\Theta\Theta') & = & \sigma\phi\chi\phi\cdots\omega\cdots\quad\cdots \end{array}$$

where η is one of the b elements, ϵ one of the c elements, &c.

We form with the circular factors of G the substitution following, with the aid of Θ in g:

$$Q=\frac{p_\alpha^A\,p_\beta^B\,p_\gamma^C\cdots p_\theta^H\,q_\eta^E\qquad\cdots\qquad r_\epsilon^e\quad\cdots}{p_1\,p_2^m\,p_3^n\cdots p_a^t\,q_{a+1}^n\,q_{a+2}^e\cdots q_{a+b}\,r_{a+b+1}^i\cdots},$$

in which the factors of the numerator are those of the denominator in a different order, so that $B=n$, if $\beta=3$ &c., and where the subindices of the numerator are the elements of Θ.

29. The effect of the substitution Q in the derived group QG is merely to change the order of entire vertical ranks of G; that is, QG is a *derived derangement* of G. In fact we see that the A vertical ranks in which the elements of the circular factor p_α repeat themselves, in QG, occupy exactly the places of those in which the circular factor p_1 repeats itself in G; the precise order of those rows being determined by the substitution

$$\frac{p_\alpha^A}{p_1}.$$

Let us next construct on Θ', in the same manner, the substitution

$$Q' = \frac{p_\kappa^k p_\lambda^l \cdot\cdot p_\rho^\zeta \cdot\cdot}{p_1 p_2^m \cdot\cdot p_a^t q_{a+1}^n}.$$

We shall have

$$QQ' = \frac{p_\sigma^s p_\phi^f \cdot\cdot p_\omega^c \quad \cdot\cdot}{p_1 p_2^m \cdot\cdot p_a^t q_{a+1}^n \cdot\cdot};$$

for in order that $\Theta\Theta'$ may be a substitution of g, it is required that

$$\Theta = \frac{\sigma\phi\chi \cdot\cdot \omega \cdot\cdot}{\kappa\lambda\mu \cdot\cdot \rho \cdot\cdot},$$

whence we must have

$$Q = \frac{p_\sigma^s p_\phi^f \cdot\cdot p_\omega^c \cdot\cdot}{p_\kappa^k p_\lambda^l \cdot\cdot p_\rho^\zeta \cdot\cdot},$$

whereby we obtain the product QQ' above written.

This proves that the substitutions

$$QQ' \cdot (QQ') \cdot\cdot$$

which are formed on those of g compose with unity a model group, for the reasons by which g is a group.

Wherefore, whatever be the system of exponents which we employ in the denominator of Q Q' &c., the same system being used in them all, and whatever be the auxiliary group g, we have always a grouped group of kl substitutions, consisting of G and $l-1$ derived derangements of G.

Let us endeavour to enumerate the groups thus constructed which shall have their principal substitutions of the form

$$fp = Aa + Bb + Cc + \cdot\cdot + Jj$$

of the principal substitutions of G.

This will restrict the number of auxiliary groups g that we can employ.

30. Let

$$\kappa\mu\pi\sigma \cdot\cdot \theta$$

be a circular factor of the order h in Θ any one of the substitutions of g, and let us suppose it formed with h of the a elements of

$$a + b + c + \cdot\cdot + j.$$

The substitution Q which we form on Θ will have the circular factor

$$\frac{p_\mu^m\, p_\pi^q \cdot\cdot\, p_\kappa^k}{p_\kappa^k\, p_\mu^m \cdot\cdot\, p_\theta^h}.$$

Let us suppose

$$\begin{aligned} p_\kappa^k &= a_\kappa\, b_\kappa\, c_\kappa \cdot\cdot \\ p_\mu^m &= a_\mu\, b_\mu\, c_\mu \cdot\cdot \\ p_\pi^q &= a_\pi\, b_\pi\, c_\pi \cdot\cdot \\ &\vdots \\ p_\theta^h &= a_\theta\, b_\theta\, c_\theta \cdot\cdot \end{aligned}$$

where the number of the elements $a_r b_r c_r \cdot\cdot$ is A.

The substitution P which is written under unity in the group G may be represented by

$$\frac{j_m}{i_m},$$

for P puts for any element i_m in a circular factor the element j_m which follows in that factor, p_m or q_m, &c.

The substitution QP is of this form:

$$\mathrm{QP} = \frac{b_\mu\, c_\pi\, d_\sigma \quad \cdot\cdot}{a_\kappa\, b_\mu\, c_\pi\, d_\sigma\, \cdot\cdot}.$$

It is plain that this circle will be closed when the circle $abcd\cdot\cdot$ of the Ath order, and the circle $\kappa\mu\pi\sigma\cdot\cdot\theta$ of the order h, are both completed; and that the order of this circle of QP will be the least common multiple of A and h. There will be A of these circles in QP beginning with b_κ, c_κ, d_κ, &c.

It follows from this view of the circular factors of QP *that the order of this substitution is the least common multiple of the orders of* Θ *and* P.

Let us suppose that P is a principal substitution of the constructed grouped group, which implies that QP shall have no circular factor of order above A made with the Aa elements, nor of order above B made with the Bb elements, &c.

It follows that h the order of the circular factor $\kappa\mu\pi\sigma\cdot\cdot\theta$

in Θ is a divisor of A, and that every circular factor in Θ made with the a or b or $c\cdot\cdot$ elements is of an order which divides A or B or C, &c. Θ is here any substitution of g.

And QP, whichever of the principal substitutions of G P may be, is a substitution of the order of P, and a principal substitution of the grouped group

$$G + QG + Q'G + \cdot\cdot$$

31. There is nothing to prevent Q from being also a principal substitution of the group constructed. If it be, we have

$$fQ = Aa + Bb + Cc + \cdot\cdot + Jj.$$

But the orders of the circular factors of Q are those of the factors of Θ, on which Q is formed. Hence Θ must have the form

$$\begin{aligned} f\Theta &= Aa_1 + Bb_1 + Cc_1 + \cdot\cdot + Jj_1 \\ &= a + b + c + \cdot\cdot + j, \end{aligned}$$

whence it appears that the partition of N on which G is formed is

$$N = A\cdot Aa + B\cdot Bb + C\cdot Cc + \cdot\cdot + J\cdot Jj.$$

When Q has the above form (fQ), every substitution of the derived group QG is a principal substitution of the grouped group; and every principal substitution $\Theta_{\prime\prime}$ of g will give a derivant $Q_{\prime\prime}$ such that $Q_{\prime\prime}G$ has k principal substitutions.

32. Let Q_a be not a principal substitution of the grouped group. The partition of N may or may not be of the form $N = A\cdot Aa$ &c. above written. If it is of this form, Qa which is not principal is constructed on Θa which is not principal in g; and Q_aG will have only R_k principal substitutions, one for every principal of G.

If the partition of N be $N = A\cdot Aa$ &c., and if the principal substitution of g has circular factors of an order below A made with the Aa_1 letters, or of an order below M made with the Mm_1 letters, no substitution Q will be

principal in the grouped group, and the only principal substitutions in the $l-1$ derived groups

$$QG, \ Q_1G, \ Q_2G, \cdot\cdot$$

will be QP Q_1P Q_2P &c., where P is any principal of G. This gives only

$$(l-1)R_k$$

principal substitutions in the $l-1$ derived groups.

If the partition of N be not of the form $N = A \cdot Aa_1$ &c., no substitution Q can be principal, and there will be only

$$(l-1)R_k$$

principal substitutions in the $l-1$ derived groups.

33. Let λ be the number of principal substitutions Θ of g which have the form, (31),

$$f\Theta = Aa_1 + Bb_1 + Cc_1 + \cdot\cdot + Jj_1;$$

then there will be $l-\lambda-1$ non-principal substitutions in g, besides unity.

The number of principal substitutions in the grouped group will be

$$R_k + \lambda k + (l-\lambda-1)R_k = lk + (l-\lambda)R_k.$$

When $\lambda = 0$, whatever be the form of the partition of N, there will be only

$$lR_k$$

principal substitutions in the grouped group.

A grouped group has its *normal form*, when it begins by the powers of a principal substitution.

The number of different groups G of theorem C, formed on the same partition of N, with which the group can commence, is

$$\frac{\lambda k + (l-\lambda)R_k}{R_k}.$$

This is the number of repetitions of the same grouped group that we shall make by selecting every one of the W groups of theorem C for our group G, without making any change in the auxiliary group g, or in the system of exponents in the denominators of the $l-1$ derivants Q

(29). Wherefore (S), the sum of our results, must be divided by this number.

34. We have to consider the effect of a variation in this system of exponents.

We can always take unity for the first exponent in the denominator of Q, of every circular factor in Q; for

$$\frac{p_\mu^M\, p_\nu^N \cdot\cdot\, p_\rho^R\, p_\kappa^K}{p_\kappa^K\, p_\mu^M\, p_\nu^N \cdot\cdot\, p_\rho^R} = \frac{p_\mu^{M-K+1}\quad p_\nu^{N-K+1}\quad\cdot\cdot\quad p_\rho^{R-K+1}\quad p_\kappa}{p_\kappa\quad p_\mu^{M-K+1}\quad p_\nu^{N-K+1}\quad\cdot\cdot\quad p_\rho^{R-K+1}}.$$

If the a factors $p_1 p_2 p_3 \cdot\cdot$ make in Q a_1 circles of the order A_1, a_2 of the order A_2 &c., and if the b factors make b_1 of the order B_1 and b_2 of the order B_2 &c., the number of different systems of exponents is

$$A^{a-a_1-a_2-\cdot\cdot}\; B^{b-b_1-b_2-\cdot\cdot}\; C^{c-c_1-c_2-\cdot\cdot}\; J^{j-j_1-j_2-\cdot\cdot};$$

for there are $a-a_1-a_2-\cdot\cdot$ of the a factors $p_1 p_2 \cdot\cdot$ which are not first in their circles, to which we can give at pleasure any one of the exponents

$$1, 2, 3, \cdot\cdot A; \text{ \&c.}$$

The first derivant Q is always constructed on Θ, a principal substitution of g, and the denominator in Q is that of all the derivants. This denominator is determined by the form of Θ, (28),

$$\begin{aligned} f\Theta = {} & A_1 a_1 + A_2 a_2 + A_3 a_3 + \cdot\cdot \quad (=a) \\ & + B_1 b_1 + B_2 b_2 + B_3 b_3 + \cdot\cdot \quad (=b) \end{aligned}$$

&c.

35. The substitution Q′ which gives the derived group Q′G is

$$Q' = \frac{p_a^\alpha\, p_b^\beta \quad\cdot\cdot\; p_1\, p_l^\lambda\, p_q^\kappa \cdot\cdot\, p_n^\nu \cdot\cdot}{p_1\, p_a^\alpha\, p_b^\beta \cdot\cdot\, p_m^\mu\, p_n^\nu\, p_l^\lambda \cdot\cdot\, p_t^\tau \cdot\cdot},$$

where

$$\frac{abc\cdot\cdot 1}{1ab\cdot\cdot m} \quad\text{and}\quad \frac{lq\cdot\cdot n}{nl\cdot\cdot t}$$

are two of the circular factors of Θ' of the orders r and s, on which Q′ is constructed, these factors being formed out of the a elements in Θ'.

The substitution $(i<k)$

$$Q'' = \frac{p_a^{a+i}\, p_b^{\beta+i} \cdots \; p_1^{i+1}\, p_l^{\lambda+i}\, p_q^{\kappa+i} \cdot\cdot\, p_n^{\nu+i} \cdot\cdot}{p_1 \;\; p_a^{a}\; p_b^{\beta} \cdot\cdot\; p_m^{\mu}\; p_n^{\nu} \;\; p_l^{\lambda} \cdot\cdot\; p_t^{\tau} \cdot\cdot}$$

is the substitution $Q'P^i$, P^i being the i^{th} substitution in order after unity in G.

The derivate $Q'G$ is also $Q''G$, (7). If we have formed $Q''G$ among our constructions made by varying our system of exponents, as the derivate Q'_1G, it will be possible to write Q'' in a form Q'_1 that differs from Q' only in the exponents of the denominator.

Now

$$Q'' = \frac{p_a^{a+i}\, p_b^{\beta+2i}\, p_c^{\gamma+3i} \cdot\cdot \;\; p_1^{1+ri} \;\; p_l^{\lambda+i}\, p_q^{\kappa+2i} \cdot\cdot \;\; p_n^{1+si} \cdot\cdot}{p_1 \;\; p_a^{a+i} \;\; p_b^{\beta+2i} \cdot\cdot\, p_m^{\mu+(r-1)i} \;\; p_n \;\, p_l^{\gamma+i} \cdot\cdot\, p_t^{\tau+(s-1)i} \cdot\cdot}$$

r and s being the orders of the factors of Θ'. And this has not the form of Q' unless

$$ri = hA$$
$$si = kA$$
$$\vdots$$

or

$$i = \frac{hA}{r} = \frac{kA}{s} = \cdot\cdot$$

In the same way we must here have

$$i = \frac{h_1B}{r_{\prime}} = \frac{k_1B}{s_{\prime}} = \cdot\cdot$$

$$i = \frac{h_{\prime\prime}C}{r_{\prime\prime}} = \frac{k_{\prime\prime}C}{s_{\prime\prime}} = \cdot\cdot$$

where $r_1 s_1 \cdot\cdot$ are the orders of the circular factors of Θ' formed with the b elements $r_{\prime\prime} s_{\prime\prime} \cdot\cdot$, those of the factors formed with the c elements, &c.

We conclude that i is not less than the least common multiple M of

$$\frac{A}{r}\frac{A}{s} \cdot\cdot \frac{B}{r_{\prime}}\frac{B}{s_{\prime}} \cdot\cdot \frac{C}{r_{\prime\prime}}\frac{C}{s_{\prime\prime}} \cdot\cdot$$

and that it may be any multiple $< k$ of this number M.

36. The group g is formed on the partition

$$\begin{aligned} a+b+\cdot\cdot+j = {} & A_1a_1+A_2a_2+\cdot\cdot \quad (=a) \\ & +B_1b_1+B_2b_2+\cdot\cdot \quad (=b) \\ & + \qquad\qquad\qquad\qquad . \\ & . \qquad\qquad\qquad\qquad . \\ & +J_1j_1+J_2j_2+\cdot\cdot \quad (=j). \end{aligned}$$

$$A_1 > A_2\cdot\cdot \quad B_1 > B_2\cdot\cdot \quad J_1 > J_2\cdot\cdot;$$

and its principal substitutions have a_1 factors of the order A_1, b_1 of the order B_1, &c.

The group g may be of the form

$$\gamma+\theta\gamma+\theta_1\gamma+\cdot\cdot,$$

where γ is a group of powers of a principal substitution of g.

If there be in g any substitutions containing circular factors of orders different from the orders $A_1\ A_2\cdot\cdot B_1B_2\cdot\cdot$ seen in the principal substitutions, those orders will all be divisors of A, of B, &c., by our hypothesis (30).

Our object is to determine the number of repetitions of grouped groups due to changes in the system of exponents of Q, the same G and g being retained.

If the system of derivates

$$QG,\ Q'G,\ Q''G,\cdot\cdot$$

made by one set of exponents, be repeated as

$$Q_1G,\ Q'_1G,\ Q_1''G,\cdot\cdot$$

made with another set,

Q_1	will be the	$(1+i)^{th}$	arrangement of	QG,
Q'_1	"	$(1+i)^{th}$	"	Q′G,
Q''_1	"	$(1+i)^{th}$	"	Q″G;

and although QG and Q′G may be repeated, Q″G will not be repeated, unless there be in Q″G a $(1+i)^{th}$ arrangement, in which $i<k$ is the least common multiple of

$$\frac{A}{r}\frac{A}{s}\cdot\cdot\frac{B}{r_{\prime}}\frac{B}{s_{\prime}}\cdot\cdot\frac{C}{r_{\prime\prime}}\frac{C}{s_{\prime\prime}}\cdot\cdot \quad (35)$$

$rs\cdot\cdot r_{\prime}s_{\prime}\cdot\cdot r_{\prime\prime}s_{\prime\prime}\cdot\cdot$ being the order of the circular factors of Θ'' on which both Q″ and Q''_1 are constructed, Θ'' being any substitution of g.

We see then that the derivates

$$QG \; Q'_1G \; Q''_1Q \cdot\cdot$$

will be reproduced by a different system of exponents, in the form

$$Q_1G \; Q'_1G \; Q''_1G \cdot\cdot$$

where Q_1 is the $(i+1)^{th}$ arrangement of Q, i being a multiple $< k$ of all the quantities

$$\frac{A}{A_1}\frac{A}{A_2} \cdot\cdot \frac{B}{B_1}\frac{B}{B_2} \cdot\cdot \frac{J}{J_1}\frac{J}{J_2}$$

where

$$A_1A_2 \cdot\cdot \; B_1B_2 \cdot\cdot \; J_1J_2$$

are the orders of all the circular factors in the group g.

Let M be the least common multiple of all these fractions.

There are in QG $\frac{k}{M}$ substitutions Q_1, each of which can be constructed as a derivant and will give a derivate (35) Q_1G identical with QG; that is, we shall, by using every possible system of exponents, repeat the same grouped group

$$\frac{k}{M}$$

times.

It is therefore necessary to divide (S), the sum of our constructions, by this number

$$\frac{k}{M}.$$

It is evident that the partition of $a+b+c+\cdot\cdot+j$, which gives the group g, is to be treated as the separate partitions

$$a = A_1a_1 + A_2a_2 + \cdot\cdot$$
$$b = B_1b_1 + B_2b_2 + \cdot\cdot,$$

if we wish to enumerate the equivalent groups g.

For the rest, g may be any group whatever, which has $A_1 A_2 \cdot\cdot$ all divisors of A, $B_1 B_2 \cdot\cdot$ all divisors of B, &c.

37. We have demonstrated the theorem following —

THEOREM H. *Let any partition of* N *be*

$$N = Aa + Bb + Cc + \cdot\cdot + Jj,$$
$$A > B,\ B > C,\ \&c.,$$

k being the least common multiple of $ABC \cdot\cdot J$, *and one at least of* $abc \cdot\cdot j$ *being* > 1.

Let G be any one of the W groups of powers of a substitution constructible on this partition of N by theorem C (12).

Let

$$p_1 p_2 \cdot\cdot p_a\, q_{a+1}\, q_{a+2} \cdot\cdot q_{a+b}\, r_{a+b+1} \cdot\cdot r_{a+b+c} \cdot\cdot$$

be the circular factors of G of the orders $ABC \cdots$

Let $p_r^i = p_r^{A+i}$ be the i^{th} cyclical permutation of p_r.

Let w equivalent groups g be constructible of l substitutions made with

$$a + b + c + \cdot\cdot + j$$

elements, of which the a elements are consecutive in unity, the b elements consecutive &c.; and such groups g that every circular factor in them made with the a elements is a divisor of A, every circular factor made with the b elements is a divisor of B, &c.; and also such groups g that every vertical row of the group written in a column shall comprise no elements but of the a or of the b &c. elements.

Let M be the least common multiple of

$$\frac{A}{A_1}\frac{A}{A_2}\frac{A}{A_3} \cdot\cdot \frac{B}{B_1}\frac{B}{B_2} \cdot\cdot \frac{C}{C_1}\frac{C}{C_2} \cdot\cdot$$

where $A_1A_2A_3 \cdot\cdot$ are the orders of the circular factors of g made with any of the a elements, $B_1B_2 \cdot\cdot$ those of factors made with the b elements &c.

Let the substitutions following be formed:

$$Q = \frac{p_\alpha^A\, p_\beta^B\, p_\gamma^C \cdot\cdot\ p_\theta^H\ q_\eta^E \quad \cdot\cdot \quad r_\epsilon^L \ \cdot\cdot}{p_1\, p_2^m\, p_3^n \cdot\cdot\ p_a\ q_{a+1}^n\, q_{a+2}^e \cdot\cdot\ q_{a+b}\, r_{a+b+1}^i \ \cdot\cdot},$$

where $\alpha\beta\gamma \cdot\cdot \theta\eta \cdot\cdot \epsilon \cdot\cdot$ is every one in turn of the $l - 1$ substitutions of the group g, $\alpha\beta\gamma \cdot\cdot \theta$ being the a elements, $\eta \cdot\cdot$ being the b elements, $\epsilon \cdot\cdot$ being the c elements, &c.; and where the terms of the numerator are those of the

denominator in a different order, so that $B=n$, if $\beta=3$ &c.; and where the exponents of the a factors p_r are anything we please >0, $<A+1$, those of the b factors q_r are anything whatever >0, $<B+1$, &c.; the same system of exponents being used in all the $l-1$ derivants Q to be formed.

The $l-1$ derived groups $Q_1G\ Q_2G \cdot\cdot Q_{l-1}G$ form with G a grouped group of Kl substitutions; and the number of equivalent grouped groups constructible on the given partition of N is

$$S=\frac{R_k MWw(A^{a-a_1-a_2\cdot\cdot}\times B^{b-b_1-b_2-\cdot\cdot}\times\cdot\cdot\times J^{j-j_1-j_2-\cdot\cdot})}{K(\lambda K+(l-\lambda))R_k}$$

where the form of the principal substitution Θ of the group g is

$$\begin{aligned} f\Theta = {} & A_1a_1+A_2a_2+A_3a_3+\cdot\cdot && (=a) \\ & +B_1b_1+B_2b_2+B_3b_3+\cdot\cdot && (=b) \\ & + \ \vdots && \ \vdots \\ & +J_1j_1+J_2j_2+J_3j_3+\cdot\cdot && (=j); \end{aligned}$$

where λ is the number of these principal substitutions of g, when their form is

$$\begin{aligned} f\Theta = {} & Aa_1 && (=a) \\ & +Bb_1 && (=b) \\ & +Cc_1 && (=c) \\ & + \\ & \ \vdots \\ & +Jj_1 && (=j), \end{aligned}$$

and where $\lambda=0$ in every other case.

R_k is here the number of integers $<k$ and prime to it, unity included.

The number of principal substitutions in each of the grouped groups is

$$\lambda K+(l-\lambda)R_k.$$

The elementary groups (ρ) of these grouped groups are groups of k substitutions, and each group (ρ) is composed of a vertical column of $\frac{k}{A}$ square groups of powers of a

substitution of A elements, or of a vertical column of $\frac{k}{B}$ square groups of a substitution of B elements &c.

These grouped groups, whose elementary groups are made up of groups of A powers, or of B powers ·· are *grouped groups of the first class.*

It is easily proved in the manner of Art. 26 that all these S grouped groups are equivalent.

38. We have restricted the auxiliary groups g that can be employed in forming the groups of theorem H (37), by the hypothesis of Art. (29). But in that article we saw that there needs be no restriction on the auxiliary group.

If this restriction be removed, and if circular factors are admitted in the group g, made with the a elements of orders which do not divide A &c. in the partition $N=Aa+$ &c., or made with the b elements of orders which do not divide B &c., we shall always construct grouped groups of kl substitutions; but they will not be in a normal form, that is, the principal substitutions of G will not be the principal substitutions of the grouped group.

For example: take

$$N=q=3\cdot 3=Aa.$$

Let G be

$$\begin{array}{c} 123456789 \\ 231698547 \\ 312874965 \end{array}$$

$$p_1=123,\ p_2=468,\ p_3=597.$$

Let the auxiliary group be

$$\begin{array}{rcc} g= & 123 & 132 \\ & 231 & 213 \\ & 312 & 321, \end{array}$$

which contains circular factors of the order 2, no divisor of A.

The grouped group constructed by five derivants Q, formed by the formula in theorem H, will contain substitutions of the sixth order, by the reasoning of Art. 30.

Let us form

$$Q_1=\frac{p_2p_3^2p_1}{p_1p_2p_3^2}=\frac{468975123}{123468975}=468937251$$

$$Q_2=\frac{p_3^2p_1p_2}{p_1p_2p_3^2}=\frac{975123468}{123468975}=975182634$$

$$Q_3=\frac{p_1p_3^2p_2}{p_1p_2p_3^2}=\frac{123975468}{123468975}=123987654$$

$$Q_4=\frac{p_2p_1p_3^2}{p_1p_2p_3^2}=\frac{468123975}{123468975}=468152739$$

$$Q_5=\frac{p_3^2p_2p_1}{p_1p_2p_3^2}=\frac{975468123}{123468975}=975436281\ .$$

We have

$$\begin{aligned}Q_1G=\,&468937251\\&684715392\\&846529173\end{aligned}$$

$$\begin{aligned}Q_2G=\,&975182634\\&759243816\\&597361428\end{aligned}$$

$$\begin{aligned}Q_3G=\,&123987654\\&231745896\\&312569478\end{aligned}$$

$$\begin{aligned}Q_4G=\,&468152739\\&684293517\\&846371925\end{aligned}$$

$$\begin{aligned}Q_5G=\,&975436281\\&759618342\\&597824163\ .\end{aligned}$$

If we arrange the grouped group

$$G+(Q_1+Q_2+Q_3+Q_4+Q_5)G$$

as a group of powers J of a principal substitution and its two derived groups, it is

123456789	597361428	684715392
684293517	123987654	597824163
312874965	759243816	468937251

468152739	312569478	759618342
231698547	975182634	846529173
846371925	231745896	975436281,

where the two derived groups are not derived derangements.

It is not easy to give an exact enumeration of the grouped groups formed by auxiliary groups g which have any circular factors whatever made with the a elements, the b elements, &c.

39. Perhaps it may simplify the conception of these grouped groups if we remark that in any group g which we select as the auxiliary group, as, for example,

$$\begin{matrix} 123 & 213 \\ 231 & 132 \\ 312 & 321, \end{matrix}$$

we may substitute for the elements 123 *any square groups of m powers* of the same number m of elements; that is, we may write above in g throughout

$$\begin{matrix} \text{for } 1, & 123; & \text{for } 2, & 456; & \text{and for } 3, & 789 \\ & 231 & & 564 & & 897 \\ & 312 & & 645 & & 978, \end{matrix}$$

and the result will be a grouped group.

It is evident that if we write the exponent 2 over the element 3 in g, the result

$$\begin{matrix} 1\ 2\ 3^2 \\ 2\ 3^2 1 \\ 3^2 1\ 2 \\ 1\ 3^2 2 \\ 3^2 2\ 1 \\ 2\ 1\ 3^2 \end{matrix} \qquad (g')$$

is still a group made with the three elements 123^2; and the substitutions $Q_1Q_2Q_3Q_4Q_5$ above formed have all 123^2, neglecting the p and reading only subindices and exponents, in the denominator, while their numerators show the remaining substitutions of (g'). It is evident also since

$$\frac{p_r^b}{p_m^a} = \frac{p_n^{b-a+1}}{p_m} = \frac{p_n^{b+4-a}}{p_m}$$

that we can remove the exponent from the denominators of $Q_1 \cdot \cdot Q_5$, if we either add $4-2$ to the exponent of the factor over p_3, or subtract $2-1$ from that exponent, without changing at all the derivants $Q_1 \cdot \cdot Q_5$. After such modification the system of subindices and exponents read in $Q_1 \cdot \cdot Q_5$, neglecting p, will be represented by

$$\begin{array}{l} 1\,2\,3 \\ 2\,3^2 1^3 \\ 3^2 1\,2^3 \\ 1\,3^2 2^3 \\ 3^2 2\,1^3 \\ 2\,1\,3, \end{array} \qquad (g'')$$

which differs from (g') only in having every exponent of the third vertical row augmented by 2, or, what is the same thing, diminished by 1.

Is now g'' a group, as well as g'? If it is, it will be unaltered, if we multiply it by any of its substitutions. We ought to have

$$(23^2 1^3) g'' = g''.$$

The rule of Art. 3 is still our guide; and this gives us $23^2 1^3$ for our first line. What then is

$$23^2 1^3 \cdot 23^2 1^3 ?$$

Our rule bids us write 2 for 1; we can then for 1^3 write nothing but 2^3. Our rule bids us next write 3^2 for 2, which is correct. We are next to write 1^3 for 3; then for 3^{1+1} we must write $1^{3+1} = 1$. This gives the product

$$(23^2 1^3)^2 = 3^2 12^3,$$

which it ought to be.

The next step is

$$23^2 1^3 \cdot 3^2 12^3 = 123,$$

and thus we easily satisfy ourselves that $23^2 1^3 \cdot g''$ is merely g'' in a different order.

If now in g'' we put

for 1,	123	for 2,	456	for 3,	789
	231		564		897
	312;		645;		978;
for 1^2,	231	for 2^2,	564	for 3^2,	897
	312		645		978
	123;		456;		789;
for 1^3,	312	for 2^3,	645	for 3^3,	978
	123		456		789
	231;		564;		897;

we obtain a group equivalent to J above written. And if for the elements of g'' we substitute

for 1,	1234	for 2,	5678	for 3,	$90ab$
	2341		6785		$0ab9$
	3412		7856		$ab90$
	4123;		8567;		$b90a$;
for 1^2,	2341	for 1^3,	3412		
	3412		4123		&c.
	4123		1234		
	1234;		2341;		

we shall complete a grouped group of 24 substitutions made with 12 elements.

If, instead of transforming g' into g'', we make the transformation

$$\begin{array}{ll} 1\,2^2 3^3 & 1\,2\,3 \\ 2^2 3^3 1 & 2^2 3^2 1^2 \\ 3^3 1\,2^2 & 3^3 1^3 2^3 \\ 1\,3^3 2^2 & 1\,3^2 2^3 \\ 3^3 2^2 1 & 3\,2\,1^2 \\ 2^2 1\,3^3 & 2\,1^3 3 \end{array}$$

by the addition of two to every exponent in the second row, and of one to every exponent in the third row, and then make in the new auxiliary thus formed the substitutions above-mentioned, we shall have grouped groups equivalent to those before formed.

The auxiliary chosen may be *any group whatever.* If

we write it in a column, then affect the element x throughout with any exponent r, not greater than the number of vertical rows in the square group we intend to substitute, and if we next so reduce the exponents in every vertical row, that unity unaffected shall surmount all, we may write everywhere for 1^m the m^{th} cyclical permutation of the first square group of R elements; for 2^q the q^{th} permutation of our second square group of R elements, &c. The result is always a grouped group, and no two groups thus obtained will be alike, though they will all be equivalents.

The mode of stating these constructions, in the enunciation of theorem H, comprehends all forms of the results for *grouped groups of the first class.*

§ 8.

Grouped groups of a higher class, whose elementary groups are of the order kr, comprising a group (g) of k powers of a substitution and r−1 derived derangements of (g).

40. Let

$$N = Aa + Bb + Cc + \cdot\cdot + Jj$$
$$A > 2,\ A > B,\ B > C,$$

&c., $abc\cdot\cdot j$ being any integers, and k being the least common multiple of $ABC\cdot\cdot J$.

The simplest of the W groups of theorem C (12) made on this partition may be represented by

$$G = (a_1 + c)(a_2 + c)\cdot\cdot(a_a + c)(\beta_1 + c)(\beta_2 + c)\cdot\cdot$$
$$(\beta_b + c)(\gamma_1 + c)\cdot\cdot(\gamma_c + c),$$

where the variable a_1 is any one of the A first elements (i) of unity, a_2 is any one of the next A elements &c., and where β_1 is any one (i) of the B elements $1_b, 2_b, 3_b\cdot\cdot B_b$, where the subindex b affects not the numerical value, and β_2 any one (i) of the B following elements, &c.

The constant c has any one of the values

$$0\ 1\ 2\cdot\cdot\quad k-1.$$

We understand by (α_m+c) (β_m+c) (γ_m+c) the residues,

$$\alpha_m+c, \text{ (mod. A)},$$
$$\beta_m+c, \text{ (mod. B)},$$
$$\gamma_m+c, \text{ (mod. C)}, \quad \&c.$$

The substitution $i+c$ of the group α_m+c is $\frac{k}{A}$ times repeated in the column

$$\begin{array}{l} i \\ i+1 \\ i+2 \\ \vdots \\ i+k-1, \end{array}$$

and every substitution $i+c$ of β_m+c is $\frac{k}{B}$ times repeated in a parallel vertical column of G.

41. Let M be any one of the numbers

$$AB \cdot \cdot J.$$

Let $y<M$ be a primitive root of the congruence

$$x^r \equiv 1 \text{ (mod. M)} \quad (r>2).$$

We can add to the group

$$(\mu_m+c)$$

$r-1$ derived derangements Art. (16), which complete with (μ_m+c) a regular group.

Let us conceive that y is a root either primitive or not of the congruence

$$x^r \equiv 1 \text{ (mod. X)},$$

X being every one in turn of the number $AB \cdot \cdot J$.

Let us add to each of the $a+b+\cdot\cdot+j$ elementary groups of G, in the same vertical column, the $r-1$ derived groups

$$y^e(\theta_m+c),$$

e being every one of the numbers $1, 2, 3 \cdot\cdot (r-1)$ and θ_m being every one of $\alpha_m\beta_m\gamma_m\cdot\cdot\cdot$ We add thus to

$$G=(\alpha_m+c)(\beta_m+c)(\gamma_m+c)\cdot\cdot$$
$$\Theta_e G=y^e(\alpha_m+c)\, y^e(\beta_m+c)\, y^e(\gamma_m+c)\cdot\cdot,$$

which has the same number of horizontal and vertical rows with G; thus adding $\Theta_1 G\ \Theta_2 G \cdots$

The $r-1$ derived groups will complete with G a group of the order kr, and they are all derived derangements of G.

For let

$$(y^e(\alpha_m+c_1))(y^e(\beta_m+c_1))(y^e(\gamma_m+c_1))\cdots \quad =\phi$$
$$(y^f(\alpha_m+c'))(y^f(\beta_m+c'))(y^f(\gamma_m+c'))\cdots \quad =\phi' \ \ (f \gtreqless 0)$$

be any two of the kr substitutions, $c_1\ c'$ being each $<k$.

The product $\Phi\Phi'$ is

$$\Phi\Phi'=(y^{e+f}(\alpha_m+c')+y^e c_1)(y^{e+f}(\beta_m+c')+y^e c_1)\cdots,$$

which is a substitution of the derived group

$$(y^{e+f}(\alpha_m+c))(y^{e+f}(\beta_m+c))(y^{e+f}(\gamma_m+c))\cdots,$$

viz. the substitution which has the value of c,

$$c=c'+y^{-f}c_1=c'+y^{r-f}c_1 \ \ (\text{mod. } k).$$

42. We obtain thus the group, (Def. Art. 2),

$$\mathrm{J}=\mathrm{G}+\Theta_1\mathrm{G}+\Theta_2\mathrm{G}+\cdots+\Theta_{r-1}\mathrm{G}.$$

If we take any other of the W groups of theorem C,

$$\mathrm{G}'=\mathrm{QGQ}^{-1},$$

we can form the group $(\Theta'_m=\mathrm{Q}\Theta_m\mathrm{Q}^{-1})$

$$\mathrm{J}'=\mathrm{G}'+\Theta'_1\mathrm{G}'+\Theta'_2\mathrm{G}'+\cdots+\Theta'^{r-1}\mathrm{G}'$$

equivalent to J, and we have thus W equivalent groups of kr substitutions by this root y.

Hence we have the theorem following—

THEOREM I. *If there be* λ *different roots* y, *of which no one is comprised among the powers of another, which fulfil the condition of Art.* (41), *we can construct*

$$\lambda\mathrm{W}$$

(*theorem* C, 12) *equivalent groups of the order* kr, *of the form*

$$\mathrm{J}=\mathrm{G}+\Theta_1\mathrm{G}+\Theta_2\mathrm{G}+\cdots+\Theta_{r-1}\mathrm{G}, \ \text{(Art. 41)}.$$

43. The number of groups constructible is much greater, if

$$y=\mathrm{E}-1, \ >1,$$

E being any one of the numbers ABC $\cdots$ J. For we can add to the group

$$\mathrm{G}=(\alpha_m+c)(\beta_m+c)\cdots(\epsilon_m+c)\cdots$$

The derived group

$\Theta_{2j+1}\mathrm{G} = (y^{2j+1}(a_m+c))(y^{2j+1}(\beta_m+c)) \cdot\cdot (y^{2j+1}(\epsilon_m+c+z_m)) \cdot\cdot$

where z_m is any one of the numbers

$$0\ 1\ 2 \cdot\cdot \mathrm{E}-1,$$

and where $z_1 z_2 z_3 \cdot\cdot$, which are introduced with the variables $\epsilon_1\epsilon_2\epsilon_3\cdot\cdot$, may be the same or different numbers. We thus have the vertical column

$$\begin{array}{l} y^\theta(\epsilon_m+z_m) \\ y^\theta(\epsilon_m+z_m+1) \\ y^\theta(\epsilon_m+z_m+2) \\ \vdots \\ y^\theta(\epsilon_m+z_m+k-1) \end{array}$$

when θ is any odd number.

The group so formed

$$\mathrm{G}+\Theta_1\mathrm{G}+\Theta_2\mathrm{G}\cdot\cdot$$

is a model group; for let

$$\Phi = (y^{(2j+1)}(a_m+c_1))\,(y^{(2j+1)}(\beta_m+c_1)) \cdot\cdot (y^{(2j+1)}(\epsilon_m+z_m+c_1)) \cdot\cdot$$
$$\Phi' = (y^{(2h+1)}(a_m+c'))(y^{(2h+1)}(\beta_m+c')) \cdot\cdot (y^{(2h+1)}(\epsilon_m+z_m+c')) \cdot\cdot$$

be any two of the kr substitutions.

Their product is

$$\Phi\Phi' = \begin{pmatrix} y^{2(j+h+1)}(a_m+c') \\ +y^{(2j+1)}c_1 \end{pmatrix} \begin{pmatrix} y^{2(j+h+1)}(\beta_m+c') \\ +y^{(2j+i)}c_1 \end{pmatrix} \cdot\cdot$$
$$\times \begin{pmatrix} y^{2(j+h+1)}(\epsilon_m+z_m+c') \\ y^{(2j+1)}z_m+c_1 \end{pmatrix} \cdot\cdot$$

Now, by the two equations

$$y^{2m} \equiv 1 \pmod{\mathrm{E}}$$
$$y^{2m+1} \equiv -1 \pmod{\mathrm{E}},$$

z_m vanishes from this product, and we have a substitution of the derived group

$$(y^{2(j+h+1)}(a_m+c))(y^{2(j+h+1)}(\beta_m+c)) \cdot\cdot (y^{1(j+h+1)}(\epsilon_m+c)) \cdot\cdot,$$

viz. that in which c has the value

$$c = c' + y^{r-(2h+1)}c_1 \pmod{k}.$$

Again: let Φ be as above, and

$$\Phi'' = (y^e(a_m+c'))(y^e(\beta_m+c')) \cdot\cdot (y^e(\epsilon_m+c')) \cdot\cdot,$$

e being any even number.

The product is

$$\Phi\Phi''=\begin{pmatrix}y^{(2j+e+1)}(a_m+c')\\+y^{(2j+1)}c_1\end{pmatrix}\begin{pmatrix}y^{(2j+e+1)}(\beta_m+c')\\+y^{(2j+1)}c_1\end{pmatrix}\cdot\cdot$$
$$\times\begin{pmatrix}y^{(2j+e+1)}(\epsilon_m+c')\\+y^{(2j+1)}(z_m+c_1)\end{pmatrix}\cdot\cdot,$$

which is a substitution of the derived group

$$(y^{(2j+e+1)}(a_m+c))(y^{(2j+e+1)}(\beta_m+c))\cdot\cdot(y^{(2j+e+1)}(\epsilon_m+z_m+c))\cdot\cdot,$$

viz. that in which

$$c=c'+y^{r-e}c_1.$$

In like manner the product

$$\Phi''\Phi=\begin{pmatrix}y^{(2j+e+1)}(a_m+c_1)\\+y^e c'\end{pmatrix}\begin{pmatrix}y^{(2j+e+1)}(\beta_m+c)\\+y^e c'\end{pmatrix}\cdot\cdot$$
$$\times\begin{pmatrix}y^{(2j+e+1}(\epsilon_m+z_m+c')\\+y^e c'\end{pmatrix}\cdot\cdot$$

is a substitution of the derived group

$$(y^{(2j+e+1)}(a_m+c))(y^{(2j+e+1)}(\beta_m+c))\cdot\cdot(y^{(2j+e+1)}(\epsilon_m+z_m+c))\cdot\cdot,$$

viz. that in which

$$c=c_1+y^{r-2j-1}c'.$$

45, We have demonstrated this theorem —

THEOREM J. *If* E *be any one of the numbers* ABC$\cdot\cdot$ *of Art.* (41), *and*

$$y=\text{E}-1$$

be a primitive root of

$$x^r\equiv 1 \pmod{\text{M}},$$

M *being any one of* ABC$\cdot\cdot$ *and at the same time a root primitive or not of*

$$x^r\equiv 1 \pmod{\text{X}},$$

X *being every one in turn of* ABC$\cdot\cdot$, *we can construct on the given partition of* N (*Art.* 41) *with this root* E − 1,

$$\text{E}^e\text{W}$$

(*theorem* C, 12) *equivalent groups of the order kr, where e is the multiplier of* E *in the partition of* N; *these groups being all of the form*

$$\text{G}+\Theta_1\text{G}+\Theta_2\text{G}+\cdot\cdot+\Theta^{r-1}\text{G},$$

where G *is any one of the* W *groups of theorem* C.

Take, as an example,

$$\text{N}=10\cdot1+8\cdot1+4\cdot2=26 \quad (k=40).$$

We have the primitive root

$$y=4-1 \text{ of } x^4 \equiv 1 \pmod{10},$$

and y is a root of the congruences

$$x^4 \equiv 1 \pmod{8}$$
$$x^4 \equiv 1 \pmod{4}.$$

We can form

$$G=(a_1+c)(\beta_1+c)(\gamma_1+c)(\gamma_2+c)$$

of the order 40.

$$\Theta_1 G = (3(a_1+c))\ (3(\beta_1+c))\ (3(\gamma_1+c+1))\ (3(\gamma_2+c+3))$$
$$\Theta_2 Q = (9(a_1+c))\ (9(\beta_1+c))\ \ (9(\gamma_1+c))\ \ (9(\gamma_2+c))$$
$$\Theta_3 G = (27(a_1+c))(27(\beta_1+c))(27(\gamma_1+c+1))(27(\gamma_2+c+3))$$

which are, treating *abcdefgh* as 12345678, and *ijklmnpq* as 12345678,

$$
\begin{array}{rl}
G = & 1\ 2\ 3\ 4\ 5\ 6\ 7\ 8\ 9\ 0\ a\ b\ c\ d\ e\ f\ g\ h\ i\ j\ k\ l\ m\ n\ p\ q \\
& 2\ 3\ 4\ 5\ 6\ 7\ 8\ 9\ 0\ 1\ b\ c\ d\ e\ f\ g\ h\ a\ j\ k\ l\ i\ n\ p\ q\ m \\
& 3\ 4\ 5\ 6\ 7\ 8\ 9\ 0\ 1\ 2\ c\ d\ e\ f\ g\ h\ a\ b\ k\ l\ i\ j\ p\ q\ m\ n \\
& \vdots \qquad\qquad\qquad \vdots \qquad\qquad\qquad \vdots \qquad \vdots \\
\Theta_1 G = & 3\ 6\ 9\ 2\ 5\ 8\ 1\ 4\ 7\ 0\ c\ f\ a\ d\ g\ b\ e\ h\ j\ i\ l\ k\ q\ p\ n\ m \\
& 6\ 9\ 2\ 5\ 8\ 1\ 4\ 7\ 0\ 3\ f\ a\ d\ g\ b\ e\ h\ c\ i\ l\ k\ j\ p\ n\ m\ q \\
& 9\ 2\ 5\ 8\ 1\ 4\ 7\ 0\ 3\ 6\ a\ d\ g\ b\ e\ h\ c\ f\ l\ k\ j\ i\ n\ m\ q\ p \\
& \vdots \qquad\qquad\qquad \vdots \qquad\qquad\qquad \vdots \qquad \vdots \\
\Theta_2 G = & 9\ 8\ 7\ 6\ 5\ 4\ 3\ 2\ 1\ 0\ a\ b\ c\ d\ e\ f\ g\ h\ i\ j\ k\ l\ m\ n\ p\ q \\
& 8\ 7\ 6\ 5\ 4\ 3\ 2\ 1\ 0\ 9\ b\ c\ d\ e\ f\ g\ h\ a\ j\ k\ l\ i\ n\ p\ q\ m \\
& 7\ 6\ 5\ 4\ 3\ 2\ 1\ 0\ 9\ 8\ c\ d\ e\ f\ g\ h\ a\ b\ k\ l\ i\ j\ p\ q\ m\ n \\
& \vdots \qquad\qquad\qquad \vdots \qquad\qquad\qquad \vdots \qquad \vdots \\
\Theta_3 G = & 7\ 4\ 1\ 8\ 5\ 2\ 9\ 6\ 3\ 0\ c\ f\ a\ d\ g\ b\ e\ h\ j\ i\ l\ k\ q\ p\ m\ n \\
& 4\ 1\ 8\ 5\ 2\ 9\ 6\ 3\ 0\ 7\ f\ a\ d\ g\ b\ e\ h\ c\ i\ l\ k\ j\ p\ m\ n\ q \\
& 1\ 8\ 5\ 2\ 9\ 6\ 3\ 0\ 7\ 4\ a\ d\ g\ b\ e\ h\ c\ f\ l\ k\ j\ i\ m\ n\ q\ p \\
& \vdots \qquad\qquad\qquad \vdots \qquad\qquad\qquad \vdots \qquad \vdots
\end{array}
$$

which is a group of the 160th order.

And we can form with this root $y=3$ sixteen equivalent groups by simply varying z_1 and z_2.

46. Each of the groups J of kr substitutions above constructed is composed of a equivalent groups $g_1 g_2 \cdot \cdot g_a$ each

of $\frac{kr}{\mathrm{A}}$ substitutions of A elements, of which every substitution is repeated $\frac{k}{\mathrm{A}}$ times; of b equivalent groups each of $\frac{kr}{\mathrm{B}}$ substitutions which are repeated $\frac{k}{\mathrm{B}}$ times, &c.

If the a equivalent groups were all made with the same A elements $123\cdot\cdot\mathrm{A}$, they could be written

$$g_1 = \frac{p_1}{p_0} g_0 \left(\frac{p_1}{p_0}\right)^{-1}$$

$$g_m = \frac{p_m}{p_0} g_0 \left(\frac{p_m}{p_0}\right)^{-1}$$

$p_0 p_1 p_2 \cdot\cdot$ being the circular factors of the A^{th} order determining the a groups $g_0 g_1 g_2 \cdot\cdot$

The a groups are in fact formed with the different sets of A elements

$$\begin{array}{l} 1\,2\,3\cdot\cdot\mathrm{A} \\ 1_1 2_1 3_1\cdot\cdot\mathrm{A}_1 \\ 1_2 2_2 3_2\cdot\cdot\mathrm{A}_2 \end{array}$$

and we can evidently still write

$$g_m = \frac{p_m}{p_0} g_0 \left(\frac{p_m}{p_0}\right)^{-1},$$

or

$$g_m \frac{p_m}{p_0} = \frac{p_m}{p_0} g_0 ,$$

if we understand that the derived group

$$\frac{p_m}{p_0} g_0$$

has lost both the elements p_0 and their order; but that the derangement

$$g_m \frac{p_m}{p_0}$$

has preserved the elements p_m and has exchanged their order for that of p_0.

With this understanding the derivate

$$\frac{p_m}{p_0} g_0$$

is identically the derangement

$$g_m \frac{p_m}{p_0}.$$

47. Let Γ be any model group of the l^{th} order made with

$$a+b+c+\cdot\cdot+j$$

elements, such that every vertical row of Γ is composed only of the a or of the b elements, &c.

Let

$$\alpha\beta\gamma\cdot\cdot, \ \theta\iota\kappa\cdot\cdot, \ \pi\rho\cdot\cdot, \ \cdot\cdot$$

be any substitution of Γ, $\alpha\beta\gamma\cdot\cdot$ being the a elements, $\theta\iota\kappa\cdot\cdot$, the b elements, $\pi\rho\cdot\cdot$ the c elements, &c.

Form the substitution

$$\mathrm{Q}=\frac{p_\alpha p_\beta p_\gamma\cdot\cdot \ q_\theta \quad q_\iota \ \cdot\cdot \ r_\pi \qquad r_\rho \ \cdot\cdot}{p_1 p_2 p_3\cdot\cdot q_{a+1}\, q_{a+2}\cdot\cdot r_{a+b+1}\, r_{a+b+2}\cdot\cdot}$$

where $q_{a+1}\,q_{a+2}\cdot\cdot$ are the circular factors of the b equivalent groups $h_1 h_2\cdot\cdot h_b$ made with the Bb elements of N &c.

The effect of the factor $\frac{p_m}{p_n}$ of Q in the derived group QJ is to change the group

$$g_n=\frac{p_n}{p_0}g_0\left(\frac{p_n}{p_0}\right)^{-1}$$

into

$$\frac{p_m}{p_0}g_0\left(\frac{p_n}{p_0}\right)^{-1}$$

which is the derangement of

$$\frac{p_m}{p_0}g_0\left(\frac{p_m}{p_0}\right)^{-1} \text{ by } \frac{p_m}{p_0}\left(\frac{p_n}{p_0}\right)^{-1}.$$

It is then evident that QJ is a derived derangement of J. If we form

$$\mathrm{J}+\mathrm{Q}_1\mathrm{J}+\mathrm{Q}_2\mathrm{J}+\cdot\cdot+\mathrm{Q}_{l-1}\mathrm{J},$$

employing all the substitutions of Γ, we shall have by the reasoning of (29) a model group of krl substitutions. In fact the effect of the derivant Q_e is to put for any elementary group of J a certain derangement of some other equivalent elementary group of J.

Wherefore we have this

THEOREM K. *Let* J *be any group of the order kr formed on the partition of* N *elements*

$$N = Aa + Bb + \cdot\cdot + Cc,$$

comprising any a equivalent groups made each of A *elements, any b equivalent groups each of* B *elements, the equivalent groups being either with or without repetition of their substitutions of the order kr; and let Γ be any group whatever of the order l made with*

$$a + b + c + \cdot\cdot + j$$

elements, of which the a first vertical rows contain only the a elements; the b next vertical rows contain only the b elements, &c.

Every pair of groups JΓ *gives a grouped group of krl substitutions, whose elementary groups are the groups of the order kr which compose* J.

The equivalent groups of the order kr may be any of the groups formed on the partition of N which have been enumerated in the preceding theorems; that is, any one of them may be determined either by one or by many circular factors.

48. It is difficult to determine how many of the grouped groups of krl substitutions thus formed may be presented as grouped groups of kr_1 substitutions formed by the process of Artt. (37, 38).

But there is an enormous number of them which cannot be so presented. If, for example, we form by theorem G (26) a group G + RG of the sixth order on the partition

$$9 = 3 \cdot 3,$$

and then add to it either two or five derived derangements by the auxiliary groups

$$\begin{array}{lll} \Gamma = 123, \text{ or } & \Gamma' = 123 & 132 \\ \quad\;\; 231 & \quad\;\; 231 & 213 \\ \quad\;\; 312 & \quad\;\; 312 & 321, \end{array}$$

we shall obtain a group of $3 \cdot 2 \cdot 3$, or of $3 \cdot 2 \cdot 6$ substitutions which cannot be arranged as a group of Artt. (37, 38), that is, as a grouped group of the first class.

§ 9.

Woven groups: Woven grouped groups.

49. It is known that, if

$$N = A + B,$$

any group G made with A elements of the order L can be interlaced with any group G′ of the order L′ made with B other elements, so as to form a woven group of LL′ substitutions made with N elements.

For example: the two woven groups

12345	6789
23145	7689
31245	6798
12354	7698
23154	
31254	

can be woven into a group of 24 substitutions.

For a second example, on the partition

$$9 = 3 \cdot 3 = Aa,$$

we can construct by aid of the auxiliary

$$\Gamma = \begin{matrix} 123 \\ 231 \\ 312 \end{matrix}$$

the grouped group G of $krl = 3 \cdot 2 \cdot 3$ substitutions, (48),

123456789	458762193	769125438
231589647	584693271	697238514
312847956	845971362	976314825
132489756	485793162	796138425
321856947	854962371	967325814
213547689	548671293	679214538,

which is G′ + RG′, (26), with two derived derangements.

G can be made into a woven group of the order 6^3 thus:

123456789	231586749	312846759
123459687	231589647	312849657
123457986	231587946	312847956
123459786	231589746	312849756
123456987	231586947	312846957
123457689	231587649	312847659, &c.

Let this group of the order 6^3 be (G); then

$$(G)\{1+458762193+769125438\}$$

is a woven grouped group of the order 648.

50. Take the partition

$$N = Aa + Bb + Cc + \cdot\cdot + Jj$$
$$A \overline{\gtrless} B,\ B \overline{\gtrless} C,\ C \overline{\gtrless} D,\ \&c.$$

Let M_R be the number of groups equivalent to any group G_R made with R elements, of S_R substitutions, and such that G_A shall not be equivalent to G_B, even though $A = B$.

The model group G_R is determined by a certain partition

$$R = R_1 r_1 + R_2 r_2 + R_3 r_3 + \cdot\cdot$$

and by a certain system of circular factors, and may be any group constructible with R elements on this partition.

Let F_a be the entire number of model groups of the order l_a constructible with a elements.

We can form of the order S_A

$$M_A^a \text{ equivalent model groups } J_A$$

with Aa elements, by selecting any one of the M_A groups G_A to be made with the first A elements $123\cdot\cdot A$, any other to be made with the next A elements of unity, &c. But this order of the elements is not necessary. We may choose the A elements a times out of Aa in

$$\frac{\pi(Aa)}{\pi a(\pi A)^a}$$

different ways. Wherefore there are, $(\pi a = 1\cdot 2\cdot 3\cdot\cdot a)$,

$$\frac{\pi(Aa)}{\pi a(\pi A)^a}(M_A)^a$$

ways of constructing a equivalent groups J_A with the first Aa elements of unity.

By theorem K (49) we can form

$$F_a(M_A)^a \frac{\pi(Aa)}{\pi a(\pi A)^a}$$

grouped groups H_A each of $S_A l_a$ substitutions with the Aa elements, each containing unity.

In like manner we can form with the Bb elements

$$F_b(M_B)^b \frac{\pi(Bb)}{\pi b(\pi A)^b}$$

grouped groups H_B with the next Bb elements, each of $S_B l_b$ substitutions; &c.

But further we need not choose the first Aa elements of unity for the groups J_A. There are

$$\frac{\pi N}{\pi(Aa)\pi(N-Aa)}$$

different ways to select the Aa elements out of N, and

$$\frac{\pi(N-Aa)}{\pi(Bb)\pi(N-Aa-Bb)}$$

ways to select from $N-Aa$ the Bb elements.

Wherefore there are

$$\frac{\pi N}{\pi(Aa)\pi(Bb)\pi(Cc)\cdot\cdot}=U$$

ways to choose the Aa the Bb the C$c\cdot\cdot$ elements, and we have

$$U\times F_a(M_A)^a\frac{\pi(Aa)}{\pi a(\pi A)^a}\times F_b(M_B)^b\frac{\pi(Bb)}{\pi b(\pi B)^b}\times\cdot\cdot$$
$$=\frac{\pi N\cdot F_aF_bC_c\cdot\cdot(M_A)^a(M_B)^b(MC)^c\cdot\cdot}{\pi a\pi b\pi c\cdot\cdot\quad(\pi A)^a(\pi B)^b(\pi C)^c\cdot\cdot}=Z$$

ways of writing under the unity of N elements a grouped group H_A of the order $S_A l_a$ made with Aa elements, a grouped group H_B of the order $S_B l_b$ made with Bb elements, &c.

Let

$$S_A l_a S_B l_b S_C l_c\cdot\cdot=\Xi;$$

and let the grouped groups

$$H_A\ H_B\ H_C\cdot\cdot$$

be formed in any one of the Z possible ways.

We can weave together these groups so as to form a woven grouped group of Ξ substitutions. But it is evident that before we weave together $H_A\ H_B\ H_C\cdot\cdot$ we can weave the a equivalent groups G_A which, with their derived derangements, compose the groups J_A of S_A substitutions. Then, instead of the $S_A l_a$ substitutions of

$$H_A = J_A + Q_1 J_A + Q_2 J_A + \cdot\cdot + Q_{l_a - 1} J_A,$$

we should have

$$\{H_A\} = \{J_A\} + Q_1\{J_A\} + Q_2\{J_A\} + \cdot\cdot + Q_{l_a-1}\{J_A\},$$

a group of the order $(S_A)^a l_a$.

Then weaving the groups

$$\{H_A\}\ \{H_B\}\ \{H_C\}\cdot\cdot$$

formed in any one of Z possible ways, we obtain a woven grouped group of

$$(S_A)^a l_a (S_B)^b l_b (S_C)^c l_c\cdot\cdot = \Omega$$

substitutions.

51. We have demonstrated the theorem following—

THEOREM L. *If* N M_R S_R F_r l_r *be the numbers defined in Art.* (49), *there are constructible on the given partition of* N

$$\frac{\pi N\cdot F_a F_b F_c\cdot\cdot F_j\ (M_A)^a\ (M_B)_b\cdot\cdot (M_J)^j}{\pi a \pi b \pi c\ \ \cdot\cdot \pi j\ (\pi A)^a\ (\pi B)^b\cdot\cdot (\pi J)^j}$$

woven grouped groups each of

$$(S_A)^a (S_B)^b (S_C)^c\cdot\cdot (S_J)^j l_a l_b l_c\cdot\cdot l_j$$

substitutions.

The largest group coustructible on this partition of N is evidently of

$$(\pi A)^a (\pi B)^b (\pi C)^c\cdot\cdot (\pi J)^j \pi a \pi b \pi c\cdot\cdot \pi j$$

substitutions. This group is *maximum*; it has no derived derangement; and the number of its derived groups is that of its equivalents.

For example: on the partition

$$10 = 3\cdot 2 + 2\cdot 2 = Aa + Bb,$$

where we have

$$M_3 = 1 = M_2 = F_2,$$

we can form

$$\frac{\Pi \cdot 10}{2 \cdot 2 \cdot 6^2 \cdot 2^2} = 6300$$

woven grouped groups, each of

$$3^2 \cdot 2^2 \cdot 2 \cdot 2 = 144$$

substitutions, if $S_A = 3$; or each of

$$6^2 \cdot 2^2 \cdot 2 \cdot 2 = 576$$

substitutions, if $S_A = 6$.

And on the partition

$$10 = 3 \cdot 1 + 3^1 \cdot 1^1 + 2 \cdot 2,$$

$$M_3 = 1, \; M'_3 = 1 = M_2 = F_2,$$

we can construct, if we take

$$S_3 = 6, \; S'_3 = 3,$$

$$\frac{\Pi 10}{1 \cdot 1 \cdot 2 \cdot 6 \cdot 6 \cdot 2^2}$$

woven grouped groups each of

$$6 \cdot 3 \cdot 2^2 \cdot 1 \cdot 1 \cdot 2 = 144$$

substitutions, all different groups from those above enumerated.

The distinctive characters of grouped groups and woven groups have, of course, been observed before. For the nomenclature, which will be found useful, I am responsible, and far from maintaining that it cannot be improved.

§ 10.

On the construction of functions of m values.

52. Let G, AG, BG··, be any group of the order L formed with N elements, and its $\frac{\Pi N}{L} - 1$ derived groups.

Let

$$P_1 = x_1^\alpha x_2^\beta x_3^\gamma \cdot \cdot x_N^\lambda$$

be the product of the N variables

$$x_1 x_2 x_3 \cdot \cdot x_N$$

raised to any N powers.

Let

$$\Phi = P_1 + P_2 + \cdot\cdot + P_L$$

where $P_1 P_2 \cdot\cdot$ are made by executing on the subindices of P_1 the substitutions of G.

It is evident that Φ will have $\frac{\Pi N}{L}$ values, if the functions $\Phi_A \Phi_B \cdot\cdot$ constructed on the derived groups AG, BG··, are all different algebraically.

When all the exponents $\alpha\beta\gamma\cdot\cdot\lambda$ are different, the functions $\Phi\,\Phi_A \Phi_B \cdot\cdot$ will be all different; for there are no two substitutions alike in the series

$$G,\ AG,\ BG\cdot\cdot$$

And since no derived group DG is identical with any equivalent of G, there will be as many distinct functions Φ, of which no one is a value of another, as there are groups equivalent to G.

If then Φ has fewer than $\frac{\Pi N}{L}$ values, we must have either

$$\Phi = \Phi_M$$

or

$$\Phi_M = \Phi_R,$$

and the number of different exponents is $< N$.

53. *a.* Let us suppose that

$$\Phi = \Phi_M,$$

or that G and MG give the same function Φ.

Since the order of the exponents is the same in all the terms (P) in Φ and in Φ_M, and since there is no substitution common to G and MG. the terms (P) in Φ_M will differ from those of Φ in the transposition of elements which carry the same exponent; that is, M exchanges only elements carrying the same exponent.

The derived group MG is either a derived derangement of G, or the derangement by M of

$$G' = MGM^{-1}$$

equivalent to G.

Suppose, first, that

$$MG = G'M.$$

There is in Φ_M a term which differs *tactically* from

$$P = x_1^\alpha x_2^\beta x_3^\gamma \cdot \cdot x_N^\lambda$$

by the exchange of certain elements

$$x_a^\theta x_c^\mu \cdot \cdot$$

for elements

$$x_b^\theta x_d^\mu \cdot \cdot$$

which carry the same exponents as before.

The same function Φ_M, both in the tactical and in the algebraic sense, is constructed on the derived MG and on the derangement G′M; and G′M differs from G′ only in the exchange of certain vertical rows carrying the exponent θ for others carrying the same θ, and of certain vertical rows carrying the exponent μ for others carrying μ, &c. Consequently the same algebraical function is constructed on G′ and on G, if the same is constructed on G and on MG which is no derived derangement of G.

Hence if G be a *maximum* group which has no derived derangement, and if the system of exponents be such that no group equivalent to G gives the same function Φ with G, we cannot have

$$\Phi = \Phi_M.$$

Suppose, secondly, that MG is a derived derangement of G.

The function Φ_M will differ tactically from Φ only in the exchange of certain vertical rows of Φ for others which carry the same exponents as before. This change leaves

$$P_1 = MP_1$$

still true algebraically, where MP_1 is the result of operating with M on the subindices of P_1.

If then the system of exponents be such that no group equivalent to G gives the same function Φ with G, and that P_1 changes in algebraic value by the operation MP_1 on its subindices, when

$$MG = GM$$

is a derived derangement of G, we cannot have

$$\Phi = \Phi_M.$$

54. *b.* Suppose, next, that

$$\Phi_M = \Phi_R,$$

or that the operation of M on the subindices of the terms (P) of Φ gives the same algebraic result with the operation thereon of R.

This is impossible, unless the effect of M on Φ, which is not also an effect of R on Φ, be limited to elements carrying the same exponent.

When this is so, we have, algebraically,

$$M^2\Phi = MR\Phi,$$

and if

$$M^r = 1$$

$$\Phi = M^{r-1}R\Phi = Q\Phi = \Phi_Q,$$

which has been proved impossible, if the system of exponents fulfils conditions above prescribed, at the end of (53).

55. We have therefore the following theorems —

THEOREM M. *Let* G *be any model group made with* N *elements of* L *substitutions. Let*

$$P_1 = x_1^\alpha x_2^\beta x_3^\gamma \cdot\cdot x_K^\theta$$

be the product of N *different powers of the* N *variables* $x_1 x_2 \cdot\cdot x_N$, *and let*

$$\Phi = P_1 + P_2 + \cdot\cdot + P_L$$

be the sum of the L *terms made by executing on* P_1 *all the substitutions* (GP) *of* G.

The function Φ *has* $\frac{\Pi N}{L}$ *values by the permutation of its variables under the fixed system of exponents. And the number of different functions* Φ *of the same degree and form, of which no one is a value of another, is the number of groups equivalent to* G.

THEOREM N. *Let* G *be any model group of* L *substitutions. Let*

$$P_1 = x_1^{\alpha} x_2^{\beta} x_3^{\gamma} \cdots x_N^{\theta}$$

where

$$\alpha \overline{\gtrless} \beta,\ \beta \overline{\gtrless} \gamma,\ \cdots\ \theta \overline{\gtrless} 0,$$

be a product which changes value algebraically by the operation MP_1 *on its subindices, when* MG *is a derived derangement of* G, *and such a product that no group equivalent to* G *gives the same function*

$$\Phi = P_1 + P_2 + \cdots + P_L$$

with G.

The function Φ *has* $\dfrac{\Pi N}{L}$ *values by the permutation of the variables under the fixed system of exponents, which values are formed on* G *and on its* $\dfrac{\Pi N}{L} - 1$ *derived groups.*

Theorem P. *Let* F *be any function symmetric or not of the* N *variables* $x_1 x_2 x \cdots x_N$, *which does not comprise the term* P_1 *above described, which gives* $\Phi = P_1 + P_2 + \cdots$, *by the group* G, *having* $\dfrac{\Pi N}{L}$ *values.*

Let

$$P_1 + F_1 = S_1,\ P_2 + F_2 = S_2,\ \&c.$$

be what

$$P + F = S$$

becomes by the substitutions of G.

The function

$$\{\Phi\} = S_1 + S_2 + S_3 + \cdots + S_L$$

has $\dfrac{\Pi N}{L}$ *values.*

56. It is of importance that we should demonstrate also the following

Theorem Q. *If two equivalent groups* G *and* G′ *give by any system of exponents the same function* Φ, *this function* Φ *has not* $\dfrac{\Pi N}{L}$ *values.*

Let us suppose that the given system of exponents is written over the elements in G and in G′, and that

$$G =_a G' =_a RGR^{-1}$$

where the symbol $=_a$ affirms *algebraic identity.*

We must have either

$$RG = {}_a G,$$

or

$$RG =\!|\!= {}_a G,$$

where $=\!|\!= {}_a$ denies algebraic identity.

If

$$RG =\!|\!= {}_a G,$$

R must exchange an element x_a which carries an exponent β in G for an element carrying a different exponent. Let us suppose that R has the substitutions

$$\frac{x_b}{x_a}$$

where x_b is an element carrying in P_1 and in G a different exponent from that of x_a.

The effect of the operation RG is to change x_a into x_b in every vertical row in which x_a appears in the function Φ written in a column, and to disturb in the same vertical rows no elements which occupy in R their natural positions.

We have the two conditions

$$RG =\!|\!= {}_a G$$
$$RGR^{-1} = {}_a G.$$

Then the derangement of RG by R^{-1} exactly compensates the algebraic disturbance produced by the operation RG. Now this derangement affects entire vertical rows of RG and can affect no row standing under an element of unity not displaced in R^{-1}, while the operation RG disturbs x_a in every row in which it appears.

It is then impossible that this compensation of algebraic disturbance can take place, and that

$$RG =\!|\!= {}_a G.$$

Wherefore

$$RG = {}_a G$$

if

$$G' = {}_a RGR^{-1} = {}_a G\,;$$

that is, G gives the same algebraic function with its

derived RG, and the function Φ has fewer than $\dfrac{\Pi N}{L}$ values. Q.E.D.

57. If we take a maximum group G consisting of a nucleus group g and all its derived derangements, and if we select a system of exponents of which some are equal to others, we have only to consider how many of the groups equivalent to G give the same algebraic function Φ.

The number of these equivalents is always given, if G be any of the groups constructed by the theorems which have preceded; and there is no difficulty in determining the groups which will give the same function.

Two equivalent groups which give the same Φ, and which have e nonrepeated exponents, can always be so arranged that the e vertical rows under those exponents shall be identical in the two groups, and that the tactical difference between the groups shall be limited to vertical rows under repeated exponents.

Let the system of N exponents chosen be α a times, β b times, γ c times repeated, &c., giving

$$N = a\alpha + b\beta + c\gamma + \cdots$$

We can readily satisfy ourselves, by inspection of the equivalent groups

$$G,\ AGA^{-1},\ BGB^{-1}\cdots,$$

or by consideration of the law that governs them, how many there are which give a function Φ not given by any other, which function may however be a value of the function given by another. And inspection of the derived derangements of these groups, or consideration of the law which governs their formation, enables us to determine how many of the groups are to be rejected for the reason that the term P does not differ in algebraic value from MP, where MG is a derived derangement of the rejected group G.

There remains a certain number r of the equivalent groups, to which the system of exponents is applicable when all those to which it is inapplicable have been rejected.

It remains to be determined how many of these r groups give *distinct functions* Φ, of which no function is a value of another.

Any one G_d of these r groups gives the same function with the derangements

$$G_d\theta_1,\ G_d\theta_2,\ G_d\theta_3\ \cdot\cdot\ G_d\theta_t, \qquad (d)$$

where

$$t=(\pi a\cdot\pi b\cdot\pi c\cdot\cdot)-1,$$

which is the number of different arrangements of N letters possible by exchange of letters bearing the same exponents. For the function Φ given by $G_d\theta_m$ cannot differ algebraically from that given by G_d.

58. It will generally be the case that G_d and $p-1$ others of the r groups have each m of these t substitutions

$$\theta_1\,\theta_2\cdot\cdot\theta_t.$$

Let us suppose that no group contains more than m of them, which are in G_d,

$$\theta_1\,\theta_2\cdot\cdot\theta_m.$$

We write

$$G_d=G_d\,\theta_1=G_d\,\theta_2=\cdot\cdot=G_d\,\theta_m \qquad (e)$$

The number of different derangements in the series (d) is in general reduced by these equations (e); that is, we have reductions of the form

$$G_d\,\theta_i=G_d\,\theta_1\,\theta_i=G_d\,\theta_j,\ \&c.;$$

and instead of $t-m$ derangements (d) we find that there are but

$$t_1-m=t',$$

and that the function Φ given by G_d is formed also on t' derangements and no more of G_d.

Now these t' derangements of G_d are each the derived of a group equivalent to G_d. We conclude that this func-

tion Φ is a value of each of the functions constructed on t' others of the r equivalent groups under consideration.

We see, further, that the same function Φ is constructed on the $m+1$ forms of the same group G_d

$$G_d \; G_d \theta_1 \;\; G_d \theta_2 \cdot \cdot G_d \theta_m ,$$

and the first term in each of these $m+1$ constructions of Φ will have the same algebraic value, since the derangements $\theta_1 \theta_2 \cdot \cdot \theta_m$ make no algebraic change in

$$P = x_1^\alpha x_2^\beta x_3^\gamma \cdot \cdot x_N^\lambda .$$

The first substitutions written in these $m+1$ forms of G_d are all different. Hence there are in G_d $m+1$ substitutions which give the same term P, that is to say, Φ has every term $m+1$ times repeated, and loses in consequence

$$\frac{mL}{m+1} \text{ terms.}$$

59. We conclude that the p groups, which contain each the same series of m out of the

$$t = (\pi a \cdot \pi b \cdot \pi c \cdot) - 1$$

substitutions, give

$$\frac{p}{t'+1}$$

distinct functions each of $\dfrac{L}{m+1}$ terms, which have each $\dfrac{\Pi N}{L}$ values.

We have thus disposed of p of the r groups, and can dispose in like manner of p' of the $r-p$ which remain. And we find in general that there are u of the r groups which contain none of the t substitutions

$$\theta_1 \theta_2 \cdot \cdot \theta_t .$$

Each one G_c of these u groups will give a function Φ_c constructible alike on t derangements of G_c, which Φ_c has L terms and $\dfrac{\Pi N}{L}$ values, and which is a value of each of the functions formed on t others of the u groups.

Thus we determine the number of distinct functions Φ

which have $\frac{L}{x+1}$ terms and $\frac{\Pi N}{L}$ values.

Nothing is gained by permuting the exponents of the term

$$P = x^\alpha x^\beta x^\gamma \cdot \cdot x^\theta.$$

If, for example, we construct Φ on

$$P' = x^\alpha x^\gamma x^\beta x^\delta \cdot \cdot x^\theta$$

we merely form Φ and its values on the derangements by $\frac{32}{23}$ of G and of its derived groups,

$$G, AG, BG, \&c.$$

In this series of derived groups we find

$$\frac{32}{23}G, \ A\frac{32}{23}G, \ B\frac{32}{23}G, \ \&c.,$$

whose derangements by $\frac{32}{23}$ are G, AG, BG·· &c.; that is, we have made the functions which are obtained by the first system of exponents, on G and its derivates.

§ 11.

Examples of the construction of m-valued functions.

60. The equivalent groups of the eighth order made with four elements are

1234	1234	1234
2341	3142	4312
3412	4321	2143
4123	2413	3421
1432	1324	1243
4321	2143	3412
3214	4231	2134
2143	3412	4321
G_1	G_2	G_3,

which are maximum groups, each consisting of a group of four powers and of its only derived derangement.

We have three distinct three-valued functions Φ, by the

system of exponents $(\alpha\beta\gamma\delta)=3210$; viz., putting $1^3 2^2 3$ for $x_1^3 x_2^2 x_3^1 x_4^0$,

$$\Phi_1 = 1^3 2^2 3 + 2^3 3^2 4 + 3^3 4^2 1 + 4^3 1^2 2 + 1^3 4^2 3 + 4^3 3^2 2 + 3^3 2^2 1 + 2^3 1^2 4,$$
$$\Phi_2 = 1^3 2^2 3 + 3^3 1^2 4 + 4^3 3^2 2 + 2^3 4^2 1 + 1^3 3^2 2 + 2^3 1^2 4 + 4^3 2^2 3 + 3^3 4^2 1,$$
$$\Phi_3 = 1^3 2^2 3 + 4^3 3^2 1 + 2^3 1^2 4 + 3^3 4^2 2 + 1^3 2^2 4 + 3^3 4^2 1 + 2^3 1^2 3 + 4^3 3^2 2,$$

The nine values of these functions of the sixth degree are all different.

Let the system of exponents be $(\alpha\beta\gamma\gamma)$.

We see that no two of the equivalent groups will give the same function, because no two can be written so as to have their two first vertical rows identical.

Hence we know that the three groups will give either different functions, or at least different values of functions.

Any one G_d of the three groups gives the same function with the derangement

$$G_d(1243).$$

The only group which contains 1243 is G_3; wherefore the function constructed on this group will be of four terms only. It is $(\alpha\beta\gamma\gamma=2100)$

$$\Phi_4 = 1^2 2 + 4^2 3 + 2^2 1 + 3^2 4.$$

The group G_1 gives the same function with its derangement

$$G_1(1243)=(1243)G_2,$$

which is a derived group of G_2. Therefore Φ_5 constructed on G_1 is a value of Φ_5 constructed on G_2. It is

$$\Phi_5 = 1^2 2 + 2^2 3 + 3^2 4 + 4^2 1 + 1^2 4 + 4^2 3 + 3^2 2 + 2^2 1.$$

Let the system of exponents be $\alpha\alpha\beta\beta$.

No two of the three groups $G_1 G_2 G_3$ will give exactly the same function, because no two can be written so as to have the same vertical row under α and the same vertical row under β, unity being $1^\alpha 2^\alpha 3^\beta 4^\beta$.

Any one of these G_d gives the same function algebraically with the derangement

$$G_d 2134 \quad G_d 2143 \quad G_d 1243.$$

G_1 has 2143; G_2 has 2143; G_3 has 2143 and 2134.

Wherefore G_3 gives a function of two terms only, which is

$$(aa\beta\beta=1100) \quad \Phi_6=12+43;$$

and G_1 gives a function of four terms which is a value of the function given by G_2, viz.,

$$\Phi_7=12+23+34+41.$$

61. The first halves of the groups G_1 G_2 G_3 are groups of the fourth order, and will give six-valued functions. They give by the system of exponents $(\alpha\beta\gamma\delta=3210)$ the halves of the functions $\Phi_1\Phi_2\Phi_3$.

The group

$$\begin{array}{l} 1234=g \\ 4312 \\ 2143 \\ 3421 \end{array}$$

gives no six-valued function by the system of $\alpha\beta\gamma\gamma$. because

$$P=x_1^\alpha x_2^\beta x_3^\gamma x_4^\gamma=1243P$$

where

$$1243g$$

is a derived derangement of g.

But the groups

$$\begin{array}{lcl} 1234 & \text{and} & 1234 \\ 2341 & & 3142 \\ 3412 & & 4321 \\ 4123 & & 2413 \end{array}$$

give each a value of the six-valued function

$$\Phi_6=1^\alpha 2^\beta 3^\gamma 4^\gamma+2^\alpha 3^\beta 4^\gamma 1^\gamma+3^\alpha 4^\beta 1^\gamma 2^\gamma+4^\alpha 1^\beta 2^\gamma 3^\gamma,$$

of which one form is

$$1^2 2+2^2 3+3^2 4+4^2 1.$$

62. The six groups each of the 20th order, made with five elements (18) give each a six-valued function by any system $\alpha\beta\gamma\delta\varepsilon$ of five exponents.

The system of four exponents $\alpha\beta\gamma\delta\delta$ gives only three functions; for each group G_d gives the same algebraic

function with its derangement $G_d 12354$, which is the derived of another group.

The three functions are ($\alpha\beta\gamma\delta\delta = 32100$)

$$\begin{aligned} V_1 = {} & 1^3 2^2 3 + 2^3 4^2 1 + 3^3 1^2 4 + 4^3 3^2 2 \\ & 2^3 3^2 4 + 4^3 1^2 3 + 1^3 4^2 2 + 3^3 2^2 1 \\ & 3^3 4^2 5 + 1^3 3^2 5 + 4^3 2^2 5 + 2^3 1^2 5 \\ & 4^3 5^2 1 + 3^3 5^2 2 + 2^3 5^2 3 + 1^3 5^2 4 \\ & 5^3 1^2 2 + 5^3 2^2 4 + 5^3 3^2 1 + 5^3 4^2 3 \end{aligned}$$

$$\begin{aligned} V_2 = {} & 1^3 2^2 3 + 2^3 5^2 3 + 4^3 1^2 3 + 5^3 4^2 3 \\ & 2^3 4^2 1 + 5^3 1^2 2 + 1^3 5^2 4 + 4^3 2^2 5 \\ & 4^3 5^2 2 + 1^3 4^2 5 + 5^3 2^2 1 + 2^3 1^2 4 \\ & 5^3 3^2 4 + 4^3 3^2 1 + 1^3 3^2 5 + 1^3 3^2 2 \\ & 3^3 1^2 5 + 3^3 2^2 4 + 3^3 4^2 2 + 3^3 5^2 1 \end{aligned}$$

$$\begin{aligned} V_3 = {} & 1^3 2^2 3 + 2^3 3^2 4 + 5^3 1^2 2 + 3^3 5^2 1 \\ & 2^3 5^2 4 + 3^3 1^2 4 + 1^3 3^2 4 + 5^3 2^2 4 \\ & 5^3 3^2 1 + 1^3 5^2 2 + 3^3 2^2 5 + 2^3 1^2 3 \\ & 3^3 4^2 2 + 5^3 4^2 3 + 2^3 4^2 1 + 1^3 4^2 5 \\ & 4^3 1^2 5 + 4^3 2^2 1 + 4^3 5^2 3 + 4^3 3^2 2 \end{aligned}$$

The eighteen values of these three functions are all different.

63. Let the system of exponents be $\alpha\beta\beta\gamma\gamma\gamma$.

Two of the six groups give values of the six-valued function

$$\begin{aligned} V_4 = {} & 1^2 23 + 2^2 53 \\ & 2^2 41 + 5^2 12 \\ & 4^2 52 + 1^2 45 \\ & 5^2 34 + 4^2 31 \\ & 3^2 15 + 3^2 24. \end{aligned}$$

The other four groups give values of the six-valued function

$$\begin{aligned} V_5 = {} & 1^2 23 + 2^2 41 + 3^2 14 + 4^2 32 \\ & + 2^2 34 + 4^2 13 + 1^2 42 + 3^2 21 \\ & + 3^2 45 + 1^2 35 + 4^2 25 + 2^2 15 \\ & + 4^2 51 + 3^2 52 + 2^2 53 + 1^2 54 \\ & + 5^2 12 + 5^2 24 + 5^2 31 + 5^2 43 \end{aligned}$$

The systems of exponents $\alpha\beta\gamma\gamma\gamma$ and $\alpha\alpha\beta\beta\beta$ are excluded, because each of the six groups gives the same symmetrical function by these systems.

The six groups of 20 (18) comprise each a group of 10. By these we can form twelve-valued functions, which are the halves of the six-valued already constructed.

64. The system $\alpha\beta\gamma\gamma\gamma$ is not excluded here, but gives by any of the groups a value of the twelve-valued function ($\alpha\beta\gamma\gamma\gamma=21000$)

$$v=1^22+2^23+3^24+4^25+5^21+2^21+1^25+5^24+4^23+3^22\,;$$

nor is the system $\alpha\alpha\beta\beta\beta$ excluded, for it gives by any of the groups a value of the twelve-valued function

$$v'=12+23+34+51.$$

The functions $V_1V_2V_3V_5v$ comprise each a twenty-four-valued function and one of its values.

There are six twenty-four-valued functions made with the system of exponents ($\alpha\beta\gamma\delta\epsilon$); three with the system ($\alpha\beta\gamma\delta\delta$), one with the system ($\alpha\beta\beta\gamma\gamma$), and one with the system ($\alpha\beta\gamma\gamma\gamma$), upon the models with which $G'_1G'_2\cdot\cdot G'_6$ begin, (Art. 18).

65. The six groups each of 120 substitutions made with six elements are thus formed, theorem F, (23).

We first write the group made with five elements

$$G'=S(pi+c)\quad(\text{mod }5)$$

of Art. 18, with the addition of six final, thus;

123456	241356	314256	432156	
234516	413526	142536	321546	
345126	135246	425316	215436	(H)
451236	352416	253146	154326	
512346	524136	531426	543216.	

We next form the derivants (23), ($\beta=2$), (20),

$$\Psi_1=\quad(2^3(i-1)^3+1)_{\text{mod. }5}\quad+\frac{1}{6}+\frac{6}{1}=645231,$$

$$\Psi_2=(2^3(1-2)^2(i-2)^3+2)_{\text{mod. }5}+\frac{2}{6}+\frac{6}{2}=465132,$$

$$\Psi_3 = (2^3(1-3)^2(i-3)^3+3)_{\text{mod. }5} + \frac{3}{6} + \frac{6}{3} = 216543,$$

$$\Psi_4 = (2^3(1-4)^2(i-4)^3+4)_{\text{mod. }5} + \frac{4}{6} + \frac{6}{4} = 532614,$$

$$\Psi_5 = (2^3(1-5)^2(i-5)^3+5)_{\text{mod. }5} + \frac{5}{6} + \frac{6}{5} = 341265.$$

The five derived groups of (H)

$(\Psi_1 + \Psi_2 + \Psi_3 + \Psi_4 + \Psi_5)$H are

645231	426531	562431	254631
452361	265341	624351	546321
523641	653421	243561	463251
236451	534261	435621	632541
364521	342651	356241	325461
465132	614532	541632	156432
651342	145362	416352	564312
513462	453612	163542	643152
134652	536142	635412	431562
346512	361452	354162	315642
216543	152643	625143	561243
165423	526413	251463	612453
654213	264153	514623	124563
542163	641523	146253	245613
421653	415263	462513	456123
532614	365214	256314	623514
326154	652134	563124	235164
261534	521364	631254	351624
615324	213654	312564	516234
153264	136524	125634	162354
341265	423165	132465	214365
412635	231645	324615	143625
126345	316425	246135	436215
263415	164235	461325	362145
634125	642315	613245	621435.

The shortest way to satisfy one's self that the 120 substitutions form a group, is to write the five derangements

$$H(\Psi_1+\Psi_2+\Psi_3+\Psi_4+\Psi_5),$$

which will be found to contain exactly the substitutions of the five derived groups above written.

The five equivalent groups to the one (J) above formed of 120 substitutions are

$$\begin{aligned}
123564J123645 &= J_1 \\
123645J123564 &= J_2 \\
123465J123465 &= J_3 \\
123654J123654 &= J_4 \\
123546J123546 &= J_5.
\end{aligned}$$

All these may be readily formed by writing out the derived groups 123564J, 123645J, &c., and then effecting a simple derangement of the three final vertical rows of the derived groups, whereby what follows is easily verified.

66. The $\pi 3=6$ groups each of 120th order, made with six elements, give six functions by the system of exponents $(\alpha\beta\gamma\delta\epsilon\zeta)$, each function of 120 terms.

The functions are reduced to three by the system of exponents $\alpha\beta\gamma\delta\epsilon\epsilon$, since each of the six groups G_d gives the same function with its derangement (Artt. 57, 58),

$$G_d 123465,$$

which is the derived of another group.

There are then three distinct functions of the tenth degree, made with the exponents $(\alpha\beta\gamma\delta\epsilon\epsilon=432100)$, each function of 120 terms.

The system of exponents $\alpha\beta\gamma\gamma\delta\delta$ gives the same algebraic function on (Artt. 57, 58)

$$G_d,\ G_d 123465,\ G_d 124356, \text{ and } G_d 124365.$$

None of the six groups has either 123465 or 124356; for none has four letters undisturbed in any substitution.

Two of the six groups have 124365; wherefore these give values of the same six-valued function of 60 terms.

The function is $(\alpha\beta\gamma\gamma\delta\delta = 321100)$

$$\begin{aligned}
Y = {} & 1^3 2^2 34 + 5^3 1^2 42 + 4^3 3^2 65 + 2^3 5^2 31 + 1^3 2^2 65 \\
& + 5^3 4^2 61 + 6^3 2^2 35 + 1^3 5^2 24 + 4^3 1^2 62 + 3^3 5^2 41 \\
& + 3^3 1^2 25 + 4^3 5^2 16 + 6^3 4^2 31 + 3^3 2^2 54 + 6^3 1^2 23 \\
& + 6^3 5^2 43 + 3^3 6^2 24 + 2^3 1^2 56 + 5^3 6^2 34 + 6^3 3^2 51 \\
& + 2^3 3^2 16 + 1^3 4^2 53 + 5^3 3^2 26 + 2^3 4^2 15 + 2^3 6^2 14 \\
& + 4^3 6^2 52 + 5^3 2^2 13 + 1^3 6^2 45 + 3^3 5^2 62 + 5^3 4^2 32 \\
& + 1^3 3^2 64 + 2^3 5^2 64 + 3^3 1^2 64 + 2^3 4^2 36 + 2^3 1^2 43 \\
& + 5^3 6^2 21 + 3^3 4^2 12 + 4^3 2^2 51 + 6^3 1^2 54 + 1^3 4^2 62 \\
& + 4^3 1^2 35 + 5^3 1^2 63 + 1^3 6^2 23 + 4^3 3^2 12 + 4^3 6^2 31 \\
& + 5^3 2^2 46 + 2^3 3^2 45 + 5^3 3^2 14 + 3^3 4^2 56 + 2^3 6^2 53 \\
& + 3^3 6^2 15 + 6^3 5^2 12 + 6^3 4^2 25 + 6^3 2^2 41 + 6^3 3^2 24 \\
& + 4^3 5^2 13 + 4^3 2^2 36 + 1^3 5^2 36 + 1^3 3^2 52 + 3^3 2^2 16.
\end{aligned}$$

The other four groups each give a value of the same function W of 120 terms of the same degree $(\alpha\beta\gamma\gamma\delta\delta = 321100)$.

67. The system of exponents $\alpha\alpha\beta\beta\gamma\gamma$ constructs the same algebraic function on G_d and on (Artt. 57, 58)

$$G_d 214365 \quad G_d 214356 \quad G_d 213465 \quad G_d 124365$$
$$G_d 123465 \quad G_d 124356 \quad G_d 213456.$$

Of the six groups there are four which have the substitution 214365, and none of the four has any of the other substitutions above written.

Wherefore these four groups all give a value of the same six-valued function of 60 terms, which is putting $(\alpha\alpha\beta\beta\gamma\gamma = 221100)$,

$$\begin{aligned}
Z = {} & 1^2 2^2 34 + 2^2 3^2 45 + 3^2 4^2 51 + 6^2 3^2 25 + 3^2 6^2 45 \\
& + 6^2 5^2 42 + 6^2 1^2 53 + 6^2 2^2 14 + 1^2 4^2 53 + 6^2 5^2 34 \\
& + 3^2 1^2 25 + 4^2 2^2 31 + 5^2 3^2 42 + 2^2 6^2 34 + 1^2 2^2 45 \\
& + 4^2 6^2 51 + 5^2 6^2 12 + 1^2 6^2 23 + 5^2 1^2 23 + 4^2 1^2 26 \\
& + 2^2 3^2 16 + 3^2 4^2 26 + 2^2 1^2 65 + 6^2 4^2 31 + 5^2 3^2 61 \\
& + 5^2 4^2 63 + 1^2 5^2 64 + 4^2 5^2 12 + 2^2 5^2 14 + 6^2 2^2 51 \\
& + 3^2 4^2 12 + 6^2 1^2 45 + 1^2 4^2 36 + 4^2 2^2 53 + 4^2 6^2 25 \\
& + 1^2 3^2 46 + 2^2 3^2 51 + 5^2 2^2 64 + 3^2 6^2 14 + 3^2 2^2 46
\end{aligned}$$

$$+2^2 5^2 63 + 6^2 3^2 12 + 1^2 4^2 25 + 1^2 3^2 65 + 5^2 1^2 62$$
$$+6^2 4^2 23 + 4^2 5^2 23 + 5^2 6^2 31 + 2^2 5^2 31 + 2^2 1^2 36$$
$$+5^2 1^2 34 + 3^2 1^2 42 + 4^2 2^2 16 + 1^2 6^2 42 + 4^2 5^2 61$$
$$+4^2 2^2 65 + 2^2 6^2 53 + 3^2 5^2 62 + 5^2 3^2 14 + 3^2 4^2 65.$$

The other two groups have the substitutions

124365 213465 214356,

and they, consequently, each give a value of the same six-valued function U of 30 terms of the degree ($aa\beta\beta\gamma\gamma$ $=221100$).

This function is

$$U = 1^2 2^2 34 + 5^2 1^2 42 + 6^2 4^2 31 + 2^2 4^2 15 + 5^2 2^2 46$$
$$+5^2 4^2 61 + 6^2 2^2 35 + 2^2 1^2 56 + 6^2 3^2 51 + 1^2 3^2 64$$
$$+3^2 1^2 25 + 3^2 6^2 24 + 5^2 3^2 26 + 2^2 6^2 14 + 5^2 6^2 21$$
$$+6^2 5^2 43 + 1^2 4^2 53 + 1^2 6^2 45 + 5^2 4^2 32 + 3^2 4^2 12$$
$$+2^2 3^2 16 + 5^2 2^2 13 + 4^2 1^2 62 + 3^2 5^2 41 + 5^2 1^2 63$$
$$+4^2 6^2 52 + 4^2 3^2 65 + 3^2 2^2 54 + 6^2 1^2 23 + 4^2 2^2 36.$$

SUPPLEMENT.

68. The preceding investigations and theorems are translated, except Art. (39) since added, from a larger work, which I last year presented to the Imperial Institute of France, in competition for their *Grand Prix de Mathématiques* for the year 1860, of which the subject was proposed early in 1858:

"*Quels peuvent être les nombres de valeurs des fonctions bien définies qui contiennent un nombre donné de lettres, et comment peut on former les fonctions pour lesquelles il existe un nombre donné de valeurs?*"

Three Memoirs were presented, but no prize was awarded. Not the briefest summary was vouchsafed of what the competitors had added to science, although it

was confessed that all had contributed results both new and important; and the question, though proposed for the first time for the year 1860, was, contrary to the *very frequent* custom of the Academy, withdrawn from competition. (*Vide Comptes Rendus,* Mars, 1861).

69. One question of the Academy as above quoted is: "*Comment peut on former les fonctions pour lesquelles il existe un nombre donné de valeurs?*"

The mode of doing this, which, so far as I can gather, has hitherto been considered satisfactory, is thus given in one of the very last contributions to this subject. (*Vide* p. 10 of *Thèses presentées à la faculté des Sciences à Paris etc., Mallet-Bachelier,* 1860; or *vide* for a reprint of the same Memoir *Le Journal de l'Ecole Polytechnique,* 1861, vol. xxii., p. 120). The author is giving an account in his preliminary chapter of the established method of constructing, on a group of the k^{th} order, with N letters, a function of $\frac{\Pi N}{k}$ values.

"Tout système conjugué ($=$*group*) peut être considéré comme le système des substitutions inaltérantes d'une certaine fonction qu'il est facile de former.

On se donne une expression dissymétrique par rapport aux N lettres $abcd\cdot\cdot$; celle-çi, par exemple, $\alpha a+\beta b+\gamma c\cdot\cdot+$; puis on forme une fonction symétrique des valeurs que prend cette expression en y effectuant toutes les substitutions du système conjugué; on prendra par exemple le produit $(\alpha a+\beta b+\gamma c\cdot\cdot)(\cdot\cdot)(\cdot\cdot)$. Cette fonction sera invariable par toutes les substitutions du système, qui ne font que permuter les facteurs les uns dans les autres; car si P, Q, R$\cdot\cdot$ sont les substitutions du système conjugué et si l'on désigne par F_x ce que devient la fonction lorsqu'on y a effectué la substitution x, le produit sera $F_1F_PF_Q\cdot\cdot$; après la substitution P il sera devenu $F_PF_{P^2}$ $F_{QP}\cdot\cdot\cdot$ Mais les substitutions P, P^2, QP$\cdot\cdot$ sont évidem-

ment distinctes, et appartiennent toutes au système conjugué: ce sont donc les substitutions $1, P, Q \cdot\cdot$ écrites dans un autre ordre. D'un autre côté toute autre substitution, altérant essentiellement les facteurs altérera le produit."

Let us try this rule on the simple group

$$\begin{matrix} abcd \\ bcda \\ cdab \\ dabc \end{matrix} \quad \text{which is} \quad \begin{matrix} 1234 \\ 2341 \\ 3412 \\ 4123. \end{matrix}$$

We may take any asymmetric function of the letters *abcd.* Take, as directed, the function

$$ad + bc.$$

The product which we are instructed to form is

$$F_1 F_P F_Q F_R = (ad + bc)(ba + cd)(cb + da)(dc + ab).$$

It is perfectly true that this is unchanged by any substitution of the group. Let us operate on it with the substitution

$$\frac{adcb}{abcd},$$

which is not in the group, and which therefore, by the last words quoted, ought to alter the product. We obtain for result,

$$(ab + dc)(da + cb)(cd + ba)(bc + ad),$$

which is the same product still. In the same manner we shall find that it is unaltered by any of the three substitutions,

$$\frac{dcba}{abcd}, \frac{cbad}{abcd}, \frac{badc}{abcd},$$

none of which is in the group, and which by the rule given ought all to alter the product.

This French rule will be found equally misleading, when applied to innumerable other groups, both simple and complex.

It would not be quite fair to lay this error to the charge of the author quoted; for he does not pretend to give any

of his own results in this first chapter. He has given abundant evidence, in the subsequent parts of his Memoir, that he is competent to deal with this difficult subject, and has here simply repeated an oversight which probably runs through previous French writers, — the oversight of this principle: that in order to prove that a function of N letters has K values, it is required not merely to shew that it is *invariable* by the substitutions of a certain group of the order $\frac{\Pi N}{K}$, but also *that it is variable for all other substitutions.*

I believe that the true theory of the connection between groups and functions to be constructed on them was first given, and I hope completely given, in the tenth section of the preceding Memoir (Art. 52).

70. I have now to correct an error of my own. This error is in the *enumeration* of equivalent groups in theorem D (14) and theorem E (17). My mistake was in the assumption, that no substitution made with N letters of the form $pi+c$ can ever be of the N^{th} order, for $p>1$. This assumption is confuted by my own remark at the end of Art. 19, page 293, that there are substitutions of the eighth order ($N=8$) of the form $5i+c$, viz. in the derivate $5iG$.

It is necessary here to determine the condition that the substitution $pi+c$ shall be of the N^{th} order, where $p<N$, >1, is prime to N, and c is any of the numbers $012\cdots(N-1)$, N being any number whatever.

We have, (Art. 15), (let me beg the reader to prefix 15 to the twentieth line of page 287),

$$(pi+c)(pi+c)(=)p^2i+pc+c(=)(pi+c)^2$$

$$(pi+c)(p^2i+pc+c)(=)p^3i+p^2c+pc+c(=)(pi+c)^3$$

$$\vdots$$

$$(pi+c)(pi+c)^{m-1}(=)p^mi+p^{m-1}c+p^{m-2}c+\cdots+pc+c$$
$$(=)(pi+c)^m,$$

where I venture to introduce the new symbol $(=)$ *of tactical equality*, to be employed when we equate a substitution C to the product of its factors, as

$C(=)AB$, or $C(=)P^m$, or $AB(=)P^m$, or $CD(=)AB$, &c.

For it will be difficult to avoid confusion, if we continue to employ the same symbol, $=$, in the tactical proposition

$$(pi+c)(qi+d)=A,$$

when we mean by A the substitution $pqi+pd+c$, and in the algebraic proposition

$$(pi+c)(qi+d)=B, \text{ or } \equiv B,$$

when we mean by B the number $pqi^2+pdi+qci+cd$.

It is above evident, that, whatever p, m, or c may be,

$$(pi+c)^m(=)p^m i+\frac{p^m-1}{p-1}c;$$

whence, if p, being prime to the composite number N, is also a primitive root of a certain congruence

$$p^m\equiv 1 \pmod{N}, \qquad (a)$$

for a value of m which is a divisor >1 of N, there is a certain number of powers of $pi+c$ of the form

$$(pi+c)^{mn}(\equiv)i+\frac{p^{mn}-1}{p-1}c(\equiv)i+e, \pmod{N}.$$

That $pi+c_1$ may be of the N^{th} order, when c_1 is prime to N, it is required, and it suffices, in addition to the conditions (a), that the congruence

$$\frac{p^t-1}{p-1}\equiv 0 \pmod{N}, \text{ (that is, } e=0)$$

be true for no value of $t<N$. When these conditions are fulfilled, we have always, for all values of $c>0<N$,

$$(pi+c)^{mn}(\equiv)i+\frac{p^{mn}-1}{p-1}c(\equiv)i+e$$

a power of θ^e, of

$$\theta(=)i+1(=)234\cdot\cdot N1,$$

and

$$(pi+c_1)^{mn}(\equiv)i, \ (c_1 \text{ prime to N}),$$

when $mn\equiv N$, and only when $mn\equiv N$.

71. When c_1 is $=1$, or prime to N, $pi+c_1$ is always of

the N^{th} order; for, if not, we shall have

$$(pi+c_1)^h (\equiv) p^h i + \frac{p^h-1}{p-1} c_1 (\equiv) i,$$

for a value of $h < N$. But as

$\frac{p^h-1}{p-1} > 0$ by hypothesis, and c_1 is $=1$, or prime to N,

$$\frac{(p^h-1)}{p-1} c_1 \equiv 0 \pmod{N}$$

is impossible. Therefore $pi+c_1$ is of the N^{th} order.

And we thus see that the entire number M of substitutions of the N^{th} order in the group $G' = S(pi+c)$ (15) is not less (12) than R_N times the number of terms in the series

$$1\, p_1\, p_2\, p_3 \cdot\cdot$$

of integers $<N$ and prime to it, including unity, which are primitive roots of the congruences

$$p^m \equiv 1 \text{ and } \frac{p^N-1}{p-1} \equiv 0 \pmod{N},$$

in which m is a divisor >1 of N.

It is easy to shew that M is not greater than that number.

The derivate piG is the derived derangement

$$piG = Gpi = P_0 + P_1 + P_2 + P_3 \cdot\cdot$$

of

$$G = 1 + \theta + \theta^2 + \cdot\cdot = S(i+c),$$

and has exactly the same vertical rows; that is, (Art. 6),

$$\frac{\theta^r}{\theta^1} = \frac{P_r}{P_1}$$

are the same substitution. If then θ and P_1 be of the same order, θ^r and P_r will be of the same order. There are then as many substitutions P_r of an order below the N^{th} as there are powers of θ below the N^{th}, which number is $N - R_N$ (12).

72. Wherefore it is proved that the number of substitutions of the N^{th} order in the group $G' = S(pi+c)$ (Art. 15) is

$$R_N \rho,$$

where ρ is the number of integers $1\, p_1\, p_2\, p_3 \cdot\cdot$, $<$N and prime to it, which are primitive roots of the congruences

$$x^m \equiv 1 \text{ and } \frac{x^N - 1}{x - 1} \equiv 0 \text{ (mod. N)},$$

in which m is a divisor >1 of N.

This is true of all the groups both of theorem D (14) and of theorem E (17). The first are all equivalents of the simple group whose NR_N substitutions are all of the form

$$pi + c, \text{ (page 287)};$$

and the number of substitutions of the N^{th} order in this group G′ (page 288), of which no one is a power of another, (viz. $\theta\chi\phi\cdot\cdot$, whose powers of the N^{th} order are

$$\theta\ \theta^a\ \theta^b\ \theta^c \cdot\cdot$$
$$\phi\ \phi^a\ \phi^b\ \phi^c \cdot\cdot$$
$$\chi\ \chi^a\ \chi^b\ \chi^c \cdot\cdot$$

&c., where $abc\cdot\cdot$ are all prime to N), is the number ρ above described.

If $\rho > 1$ in G′, it is plain that no equivalent to G′, $\mathrm{QG'Q^{-1}}$, can have the powers of

$$\theta = i + 1 = 234\cdot\cdot\mathrm{N}1;$$

for every substitution of $\mathrm{G'} = \mathrm{S}(pi + c)$ is given with $\theta = i + 1$, whose powers compose the model G (page 287). (Let me here beg the reader to complete the ninth line of page 287 thus: The simplest of these groups is formed on the model G).

It follows that none of the substitutions, $\phi\ \chi$ &c. of the N^{th} order in G′, can occur in any equivalent of G′; for G′ can be equally written as the model

$$1\ \phi\ \phi^2\ \phi^3 \cdot\cdot$$

and $\mathrm{R}_N - 1$ derived derangements of it; or as the model

$$1\ \chi\ \chi^2\ \chi^3 \cdot\cdot$$

and $\mathrm{R}_N - 1$ derived derangements of it, &c.

But every equivalent of G′ will contain, like G′, ρ sets of powers of distinct substitutions of the N^{th} order, none of

which sets will be found in any other equivalent. We obtain, therefore, the number of equivalent groups by dividing by ρ that given in theorem B (12) of the equivalent groups of powers of a substitution of the N^{th} order.

73. The correction required in theorem D, (14), is simply to write $\frac{\Pi(N-1)}{R_N \cdot \rho}$ instead of $\frac{\Pi(N-1)}{R_N}$ in the first line of it, adding the definition, *that ρ is the number of integers, unity included, which are $<N$ and prime to it, and which are primitive roots of the congruences*

$$x^m \equiv 1 \text{ and } \frac{x^N-1}{x-1} \equiv 0 \text{ mod N},$$

in which m is any divisor >1 of N.

If the paragraph beginning at line nine and ending at line fifteen of page 288 be erased, the fourteenth and fifteenth articles are corrected.

The groups of theorem E are all equivalent to the simple group

$$G'' = S(pi + c),$$

in which p is every power of any primitive root of the congruence chosen

$$x^r - 1 \equiv 0 \text{ (mod. N)}.$$

The correction required at page 289 is to write in the nineteenth, twenty-fifth, thirtieth and thirty-first lines,

$$\frac{\Pi(N-1)}{R_N\rho} \text{ for } \frac{\Pi(N-1)}{R_N},$$

and to add the definition, *that ρ is the number of the integers $1\, p\, p^2 \cdot \cdot p^{r-1}$, which are powers of the primitive root employed of the congruence $x^r - 1 \equiv 0$, when r is a divisor of* N, *and which are roots of no congruence*

$$\frac{x^m-1}{x-1} \equiv 0 \text{ (mod. N)},$$

in which $m <$ N; and that $\rho = 1$, when r is not a divisor of N.

We have $\rho = 1$, if N is a prime number, in both theorems.

The theorem F (page 293) requires no correction.

74. It is not necessary for me to determine the form of N when $\rho > 1$. But I suspect that the only possible form is

$$N = n^{2+h},$$

where n is a prime number.

We know that, in all the groups of theorems D and E, no substitution

$$pi + c$$

is of the N^{th} order, even when $\rho > 1$, if c be a factor of N; for it is found in $PG = P_1 + P_2 + P_3 + \cdot\cdot$, a derived derangement of

$$G = S(i + c) = 1 + \theta + \theta^2 + \cdot\cdot,$$

and has the same vertical rows with G, of which every substitution can be written, (6, page 279),

$$\frac{\theta^a}{\theta} = \frac{P_a}{P_1};$$

wherefore if θ and P_1 are of both the N^{th} order, θ^a and P_a must be both of one order. When p is one of the ρ numbers, $P_1 = pi + 1$ is of the N^{th} order, like $\theta = i + 1$; and as θ^a, when a is not prime to N, is of an order below the N^{th}, P_a is not of the N^{th} order. Therefore

$$P_a = pi + a$$

can be raised to the power unity,

$$P_a^{rj} (=) (pi + a)^{rj} (\equiv) p^{rj} i + \frac{p^{rj} - p}{p - 1} a (\equiv) i,$$

where $rj < N$, and where $p^r \equiv 1$, (mod. N).

And as

$$\frac{p^{rj} - p}{p - 1} > 0 \text{ for } rj < N,$$

we must have

$$\frac{p^{rj} - p}{p - 1} \equiv \frac{N}{a},$$

which may be *any factor whatever* of N, $< N$, > 1.

I think it will be proved that N and a must of necessity be powers of the same prime number.

75. It is worth remarking, by way of supplement to the

example of Art. (19), that when $N=8$, 5 is the only primitive root >1 of the congruence

$$\frac{x^8-1}{x-1}\equiv 0 \pmod{8},$$

and we have

$$5^2\equiv 1 \pmod{8};$$

whence $\rho=2$, and there are (theorem D) $\frac{\Pi 7}{8}$ equivalent groups of 32, each comprising two sets of powers of substitutions $\theta\phi$ of the eighth order, which have however common powers

$$\theta^a=\phi^a$$

when a is a factor of 8.

It follows by theorem A, (9), that there are $\frac{\Pi 7}{8}$ equivalent groups each of $\frac{\Pi 8\cdot 8}{\Pi 7}=64$ made with eight elements.

When $N=9$ we have only $p=4$ and $p_1=7$ primitive roots >1 of the congruences

$$x^3\equiv 1 \text{ and } \frac{x^9-1}{x-1}\equiv 0 \pmod{9}.$$

Hence $\rho=3$, and there are $\frac{\Pi 8}{6\cdot 3}$ (theorem D) equivalent groups of 54; whence by theorem A there are $\frac{\Pi 8}{6\cdot 3}$ equivalent modular groups each of $\frac{\Pi 9\cdot 6\cdot 3}{\Pi 8}=9\cdot 6\cdot 3$, made with nine elements.

When $N=16$ we have only $p=5$, $p_1=9$, $p_2=13$, primitive roots >1 of the congruence

$$x^4\equiv 1 \text{ and } \frac{x^{16}-1}{x-1}\equiv 0 \pmod{16};$$

whence $\rho=4$, and we have (theorem D) $\frac{\Pi 15}{8\cdot 4}$ equivalent groups each of $16\cdot 8$ substitutions, and consequently by theorem A, $\frac{\Pi 15}{8\cdot 4}$ equivalent modular groups each of $\frac{\Pi 16\cdot 8\cdot 4}{\Pi 15}=16\cdot 32$ made with sixteen elements.

I proceed to give a brief sketch of several investigations, all of interest, and most of them of new interest, in this theory, which I hope soon to discuss completely in a second Memoir.

76. *On the didymous factors of the substitution* ϕ.

With every substitution ϕ made with

$$N = Aa + Bb + Cc + \cdot\cdot Jj$$

letters, of the order K, which is the least common multiple of $ABC \cdot\cdot J$, there is given a certain number of systems of K square roots $a_1 a_2 a_3 \cdot\cdot a_K$, such that the product of any two $a_m a_n$ is a power of ϕ; and every power of ϕ can be written as the product of K different pairs of such *didymous radical factors* of the system.

Thus, with the group of powers

$$i, (i+1), (i+2), (i+3 \cdot\cdot, (i+N-1), = 1, \phi, \phi^2, \phi^3 \cdot\cdot,$$

there is given the system of N square roots of unity

$$\underset{a_1}{(2-i)}, \underset{a_2}{(1-i)} \underset{a_3}{(-i)} \underset{a_4}{(-i-1)} \underset{a_5}{(-i-2)} \cdot\cdot \underset{a_N;}{(-i-(N-3))}$$

and we have, (putting $1-i$ for i in a_1 for the product $a_1 a_2$),

$$a_1 a_2 (=) i+1, \; a_1 a_3 (=) i+2, \; a_1 a_4 (=) i+3, \; \&c.$$

$$a_2 a_3 (=) i+1, \; a_2 a_4 (=) i+2, \; a_2 a_5 (=) i+3, \; \&c.$$

$$a_3 a_4 (=) i+1, \; a_3 a_5 (=) i+2, \; a_3 a_6 (=) i+3, \; \&c.$$

It is easy to form the system of such radicals corresponding to any substitution when the partition is

$$N = N \cdot 1 = Aa,$$

and ϕ is of the N^{th} order. Take, for example, the substitution 57683241 of the eighth order.

We form the skeletons

12345678	12438765 $= a_1$
$\phi =$ 57683241	8
3	4
6	7
2	2
7	6
4	3
8	5

in which the first vertical circle is given by 57683241, the second is the same circle reversed, and

$$a_1 = \frac{18472635}{15362748}.$$

We next complete the vertical circles thus:

	12345678	12438765	a_1
ϕ	57683241	86754231	a_2
ϕ^2	34216785	43217658	a_3
ϕ^3	68752413	75682314	a_4
ϕ^4	21437856	21346587	a_5
ϕ^5	75864132	68573142	a_6
ϕ^6	43128567	34125876	a_7
ϕ^7	86571324	57861423	a_8.

We see that

$$\phi\,(=)a_1a_2(=)a_2a_3(=)a_3a_4\cdots(=)a_7a_8(=)a_8a_1$$
$$\phi^2(=)a_1a_3(=)a_2a_4(=)a_3a_5,\ \text{\&c.}$$
$$\phi^3(=)a_1a_4(=)a_2a_5(=)a_3a_6,\ \text{\&c.}$$
$$\vdots$$
$$\phi^7(=)a_2a_1(=)a_3a_2(=)a_4a_3,\ \text{\&c.}$$

And there are sixteen equations of the form

$$a_2a_1a_2(=)a_3,\quad a_3a_2a_3(=)a_4,$$
$$a_8a_1a_8(=)a_7,\quad a_1a_2a_1(=)a_8,\ \text{\&c.},$$

or of the form $a_q\,a_p\,a_q(=)a_r$, where $p\,q\,r$ are consecutive radicals of the system.

We shall speak of a_1, a_2, $a_3 \cdot\cdot$ as of the *didymous radicals of ϕ, that is, of the group of powers of ϕ*, and any pair a_ma_n, giving $a_ma_n = \phi^i$ are *didymous factors* of ϕ^i.

The whole group both of powers and radicals is given with any two consecutive radicals. Thus, if $a_1 = 143256$, $a_2 = 213546$, we have the skeletons

123456	143256
413526	213546
5	5
2	4

where

$$143256 \times 213546 (=) 413526 ;$$

and we complete the group of eight thus:

$$\begin{array}{llll} & 123456 & 143256 & a_1 \\ \phi & 413526 & 213546 & a_2 \\ \phi^2 & 543216 & 523416 & a_3 \\ \phi^3 & 253146 & 453126 & a_4 . \end{array}$$

This, as well as the group of sixteen preceding, is one of the groups enumerated in theorem G, (26).

The partition of N is here

$$6 = 4 \cdot 1 + 1 \cdot 2, \text{ and } K = 4.$$

77. *Whenever the order* K *of the group* $1 + \phi + \phi^2 \cdot \cdot$ *is even, the didymous factors* $a_c a_e (=) \phi^{2m}$ *are always two of the same form, but those of* ϕ^{2m+1} *are two of different forms.*

Thus a_1 and a_3 are of the form

$$6 = 2 \cdot 1 + 1 \cdot 4 = Aa + Bb,$$

whilst a_2 and a_4 are of the form

$$6 = 2 \cdot 2 + 1 \cdot 2 = Aa + Bb.$$

When the order K *of the group is odd, the didymous radicals of the system are all of one form.*

An important point is here to be noticed concerning the radicals of such a group as the one last written, formed on the partition

$$N = Aa + 1 \cdot 2,$$

where the group $1, \phi, \phi^2 \cdot \cdot$ has two letters undisturbed. The derived derangement RG of theorem G, (26), will consist of radicals having the same two letters undisturbed. But if we exchange those two letters, writing, for example, above,

$$\begin{array}{llll} 146253 & a'_1, \text{ for } & 143256 & a_1 \\ 216543 & a'_2 & 213546 & a_2 \\ 526413 & a'_3 & 523416 & a_3 \\ 456123 & a'_4 & 453126 & a_4 , \end{array}$$

we have still a system of didymous radicals such that the product of any two is a power of ϕ. The form $a'_1 a'_3$ is

$$6 = 2 \cdot 2 + 1 \cdot 2,$$

and that of $a'_2 a'_4$ is

$$6=3\cdot 2.$$

And these are the forms which are always seen in the didymous radicals of a substitution ϕ of the sixth order. For example:

$a'_1=146253$	and	$216543=a'_2$
351624		423165
423165		645231
564312		361452
215436		532614
632541		$154326,$

of which the first set contains a'_1 and the other a'_2, are didymous radicals of the powers of $\phi_1=364521$, and of $\phi_2=245613$, both of the sixth order.

This modification of the derived derangement RG is not taken into account in the enumeration given by the theorem G, which however gives correctly the number of equivalent groups of the precise form $R+RG$ therein specified. If RG becomes R′G by the modification here noticed, $G+R'G$ is a form of group not specified in that theorem, of which the equivalents can be enumerated.

This requires further developement, for which I have not space here at my disposal.

78. *Tactical investigation of the groups of* $N\,\overline{N-1}\,\overline{N-2}$ *substitutions formed with* N *elements, when* $N-1$ *is a prime number* (*theorem* F, 23).

By a *tactical investigation* I mean one in which no numerical equations or congruences are necessarily used.

The discovery of these groups was first published by M. Mathieu; and they were both found and enumerated by myself before I was aware that he had secured a prior claim. His demonstration is effected by an analysis of formidable difficulty (*vide* Liouville's *Journal*, January, 1860); and my own (20), though more readable than his, as avoiding imaginaries, is sufficiently abstruse.

The following investigation of M. Mathieu's theorems, when $N-1$ is the power of any prime number, will be probably found simpler than either.

We begin first with $N-1$, a prime number.

By a theorem of Cauchy's, we can form with the $N-1$ elements $123\cdot\cdot(N-1)$ a group G of $(N-1)(N-2)$ substitutions all of the form

$$G=S(pi+c),$$

where p has any of the $N-2$ values

$$123\cdot\cdot N-2,$$

and c any of the $N-1$ values

$$0123\cdot\cdot N-2,$$

by writing, for any element i of unity,

$$pi+c, \text{ (mod. } N-1).$$

In this group G there are $N-2$ substitutions ending in $(N-1)$, which form a group g of the order $N-2$; and G contains also g_r, a group of the same order, in which the element r is undisturbed. The substitutions of this group g_r are

$$S(pi+r-pr).$$

Thus G can be written as the product of the group

$$g_0=S(i+c)$$

of $N-1$ powers,

$$1+\theta+\theta^2+\cdot\cdot+\theta^{N-2},$$

where

$$\theta=2345\cdot\cdot\overline{(N-1)}1,$$

and of $N-1$ groups g_r of the order $N-2$, in each of which one element is undisturbed. That is,

$$G=g_0g_1g_2\cdot\cdot g_{N-1}.$$

The $N-1$ didymous radicals of g_0 are

$$a_0b_0c_0d_0\cdot\cdot$$

where

$$a_0b_0=\theta,\ a_0c_0=\theta^2,\ a_0d_0=\theta^3,\ \&c.$$
$$b_0c_0=\theta,\ b_0d_0=\theta^2,\ b_0e_0=\theta^3,\ \&c.$$
$$c_0d_0=\theta,\ c_0e_0=\theta^2,\ \&c.$$

And these $N-1$ radicals are the central powers of the

groups g_r; that is, if

$$g_1 = 1, \text{A}, \text{A}^2, \cdot\cdot \text{A}^{N-3},$$

we have

$$a_0 = \text{A}^{\frac{1}{2}(N-2)}.$$

Let the N − 1 groups $g_1 g_2 \cdot\cdot$ be written, and let each group have written under it its N − 2 didymous radicals, viz.,

$$a_1 a_2 a_3 \cdot\cdot a_{N-2} \text{ under } g_1,$$
$$b_1 b_2 b_3 \cdot\cdot b_{N-2} \text{ under } g_2, \text{ \&c.}$$

We have a system of N − 1 groups $h_1 h_2 h_3 \cdot\cdot$ of the order 2(N − 2) in each of which, h_r, is a didymous factor of θ, viz. r_0, which is permutable with each of the N − 2 didymous radicals of h_r.

79. *Every group of m powers*

$$1\ \phi\ \phi^2 \cdot\cdot \phi^{m-1},$$

when m is even, has one power $\phi^{\frac{1}{2}m}$ *permutable with each of the m didymous radicals of the group.*

It is easily seen that if h_r be any one of the N − 1 groups $h_1 h_2 h_3 \cdot\cdot$

$$\theta h_r \theta^{-1},\ \theta^2 h_r \theta^{-2}, \cdot\cdot\ \theta^{N-1} h_r \theta$$

are the remaining N − 2 groups. That is, if γ be any one of the (N − 1)(N − 2) didymous radicals in the groups $h_1 h_2 \cdot\cdot$ and δ be any other, we have always

$$\gamma = \theta^a \delta \theta^{-a}, \text{ or}$$
$$\theta^a \gamma = \theta^a \delta,$$

for some value of a.

The didymous radicals in the group h_m are alternately of the two forms, (Art. 27),

$$\text{N} - 1 = 2 \cdot \frac{\text{N} - 4}{2} + 1 \cdot 3 \ = \text{A}a + \text{B}b$$

$$\text{N} - 1 = 2 \cdot \frac{\text{N} - 2}{2} + 1 \cdot 1, = \text{A}a + \text{B}b,$$

and those in the group h_r have all the element r undisturbed.

Let us denote by H the entire system of (N − 1)(N − 2)

substitutions of G along with the $(N-1)(N-2)$ didymous radicals of $h_1 h_2 \cdot\cdot$ made with $N-1$ elements, $123\cdot\cdot(N-1)$.

If we add N as a final element to each of these $2(N-1)(N-2)$ substitutions, G so augmented is still G′ a group of $(N-1)(N-2)$, and each of the augmented groups h'_r is still a group of $2(N-2)$ substitutions, containing the augmented g'_r.

And it is easily proved that every pair xy of the first $N-1$ elements is found once, and once only, in the order xy in the c^{th} and d^{th} vertical rows of H written in a column, and once, and once only, in the order yx in the same two vertical rows of H, whatever c and d may be $<N$.

If further we exchange in all the didymous radicals of g'_r the elements N and r, they remain still didymous radicals of the augmented group g'_r.

Let the system H so modified become H′.

It remains true of the group $h'_1 h'_2 h'_3 \cdot\cdot$, so modified, that if h'_r be any one of them, the others are

$$\theta h'_r \theta^{-1},\ \theta^2 h'_r \theta^{-2},\ \theta^3 h'_r \theta^{-3},\ \&c.,$$

where

$$\theta = 2345\cdot\cdot(N-1)1N;$$

that is, all the didymous radicals in the modified system H′ are such that if a be any one of them,

$$\theta^c a \theta^{-c} = \beta$$

is another, similar to a, whatever c may be.

But the $(N-1)(N-2)$ didymous radicals of the system H′ are now alternately of the two forms,

$$N = 2\frac{N-2}{2} + 1\cdot 2 \quad = Aa + Bb,$$

$$N = 2\cdot\frac{N}{2}, \quad = Aa;$$

that is, they are of the forms always read in the didymous radicals of the powers of a substitution of the order N.

80. The group G', with N final throughout, can be written as the modular group of $(N-1)(N-2)$ substitutions,

$$G'=g'_0+Ag'_0+A^2g'_0+\cdots+A^{N-3}g'_0,$$

where Ag'_0 &c. are $N-3$ derived derangements of $g'_0 = 1+\theta+\theta^2+\cdots$

$$(\theta=234\cdots(N-1)1N)$$

by the substitutions of g'_1.

This group G' consists of the cyclical permutations of the $N-1$ first places, of unity, of A, of A^2, of A^3, &c. The didymous radicals of h'_1 are

$$a_1,\ a_2=a_1A,\ a_3=a_1A^2,\ a_4=a_1A^3,\ \&c.$$

If then we write under a_1,

$$a_1\theta,\ a_1\theta^2,\ a_1\theta^3\cdots,$$

the cyclical permutations of the first $N-1$ places of a_1; under a_2,

$$a_2\theta,\ a_2\theta^2,\ a_2\theta^3\cdots;$$

and under a_3,

$$a_3\theta,\ a_3\theta^2,\ a_3\theta^3\cdots,$$

&c.; we shall form a_1G' the derivate of G' by a_1, consisting of $(N-1)(N-2)$ substitutions all ending in 1. And this is the derivate of G' by every substitution in a_1G'.

In the same way we can complete the derivates b_1G', c_1G', &c. of G' by b', by c', &c., which have a different final vertical row of one letter. For G' can be written

$$G'=g'_0+Bg'_0+B^2g'_0+\cdots,$$

where $BB^2B^3\cdots$ are the substitutions of g'_2.

81. It can be proved, by a simple tactical argument founded on the property stated in Art. 79, concerning the position of every duad xy, that if a_0 and b be any two of the square roots of H', $ba_0b=c$ is another.

Let b be written below a_0, and under b write the substitution

$$b\frac{a_0}{b}=c;$$

then c is the third of the series $a_0bc\cdots$ of the didymous

radicals of a certain substitution ϕ. We have (Art. 76),

$$\frac{a_0}{b}=a_0b=\phi=bc=cd=de, \text{ \&c.}$$

$$ba_0b=c, \; cbc=d, \; dcd=e, \text{ \&c.}$$

Hence all the series $a_0bc\cdot\cdot$ can be proved to be present in H′.

Let β be any primitive root of N − 1. Then one of the sets of N − 2 radicals is always obtained by writing as the first line the substitution

$$\frac{1\beta\beta^2\beta^3\cdot\cdot\beta^{N-4}\beta^{N-3}}{1\beta^{N-3}\beta^{N-4}\cdot\cdot\beta^2\beta}, \text{ (mod. (N − 1))},$$

made with N − 2 elements, and completing under each element the vertical circle

$$1\beta^{N-3}\beta^{N-4}\cdot\cdot\beta^2\beta, \text{ (mod. (N − 1))}.$$

We then add the elements N and N − 1 to each of the radicals so obtained.

If under any one (b) of the $\frac{1}{2}$(N − 2) thus found, which have no letter undisturbed, we write the didymous radicals of

$$\theta=2345\cdot\cdot(N-1)1N,$$

viz.

$$1\,\overline{N-1}\;\overline{N-2}\cdot\cdot\quad 432N \qquad (a_0)$$

$$\overline{N-1}\;\overline{N-2}\;\overline{N-3}\cdot\cdot\quad 321N \qquad (b_0)$$

&c., we always find more than one of the quotients

$$\frac{b}{a_0}, \frac{b}{b_0}, \frac{b}{c_0}, \frac{b}{d_0}\cdot\cdot,$$

which are substitutions of the order N.

Let $\phi=ba_0=\frac{b}{a_0}$ be of the order N; then we have present in H′ the series

$$a_0bcd\cdot\cdot$$

of N didymous radicals of ϕ; and as no two of $bcd\cdot\cdot$ end in N, and as no two end with the same element, they are found one in each of the N − 1 groups $h'_1h'_2h'_3\cdot\cdot$; and the N − 1 derivates of G′ above formed are

$$bG', \; cG', \; dG', \text{ \&c.}$$

Now

$$bG' \text{ contains } ba_0 = \phi,$$
$$cG' \text{ contains } ca_0 = \phi^2,$$
$$dG' \text{ contains } da_0 = \phi^3,$$

&c.; that is, the N – 1 derivates are

$$\phi G', \ \phi^2 G', \ \phi^3 G' \cdot\cdot \phi^{N-1} G'.$$

This, along with what has been already proved, that every substitution of these derivates is of the form

$$a_m \theta^r = \theta^r c_n,$$

completes the *tactical demonstration* that

$$K = G + \phi G + \phi^2 G + \phi^3 G + \cdot\cdot \phi^{N-1} G$$

is a group of N(N – 1)(N – 2) substitutions.

All these groups K (Art. 23) are *non-modular*, (9).

82. This group (K) contains N different systems H constructible on N different groups g_0 of the $(N-1)^{th}$ order, which have each a different letter undisturbed. They all agree in this, that if the N – 1 didymous radicals of the group g_0 be added to those of the groups $g_1 g_2 \cdot\cdot$ each of N – 2, after an exchange, under every one g_r, of the fixed letter of g_0 for r, we have in every case the same $(N-1)^2$ substitutions of the second order.

The connection between these systems H and the groups of the N^{th} order comprised in K lies herein, that these have exactly the same $(N-1)^2$ square roots for their $\left(\frac{N}{2}\right)^{th}$ powers, and for their didymous radicals.

The $\frac{\pi(N-1)}{R_N}$ groups (L) of the N^{th} order are symmetrically distributed among the $\pi(N-3)$ groups K; that is, for N = 6, the sixty groups L are found ten together in the six groups K. For N = 8, the $\frac{\pi 7}{4}$ groups L are found twenty together in the 120 groups K, &c.

83. *Applications of theorems* H, (37), *and* A, (9).

The existence of modular groups has long been known. I know not whether Betti or Cauchy first gave the general

theorem, *that if to a group* G *you add any number of derived derangements of* G,

$$PG + QG + RG \ \&c. = GP + QQ + QR + \cdot\cdot,$$

such that every power and product of the derivants PQR·· *is found in the series* PQR··, *the sum* M *of substitutions so obtained is a group.*

This principle has remained since the days of Cauchy little more than a broad and barren generality. It is not an easy step from this proposition to the answer of the following questions:

I. How many derived derangements at the most can be added to the model group G?

II. How many different inferior modular groups can be selected from the maximum modular group?

III. How are we to form these modular groups, both maximum and subordinate?

IV. What is the number in each case of the possible equivalent groups?

V. How are we to determine, when a non-modular group, which is not simply a group of powers of ϕ, is found, the number and order of modular groups of which it is the model?

I am not aware that these questions have been discussed elsewhere than in this Memoir.

The first and the last of these questions are, if I mistake not, sufficiently answered by my theorems, including the corollary to theorem A, (9); and this corollary, along with the theorem H, (37), is an instrument of new and considerable power, as we shall presently see.

84. As an example, which will introduce certain important groups that are known, and a superior group which is probably new, let us take the partition

$$8 = 2 \cdot 4 = A \cdot A^2 = Aa.$$

The theorem H, (37), gives thirty for the number of equivalent grouped groups (F) of eight substitutions, all

except unity principal and of the second order. We have here

$$M=1=R_K, \ W=105, \ K=2, \ l=4, \ \lambda=3=w,$$

$$S=\frac{1\cdot 1\cdot 105\cdot 3(2^{4-2})}{2(3\cdot 2+1)}=30.$$

By the corollary of theorem A, (9), it follows that there are

$$\frac{\pi 8}{30}=8\cdot 7\cdot 4\cdot 3\cdot 2$$

substitutions in each of the thirty equivalent maximum modular groups V made on this partition.

Hence there are thirty distinct transitive functions of thirty values made with eight letters, of the same degree, (57), *that is, functions formed on transitive groups.*

The next and the more difficult question is, How is this maximum group V to be formed on the model F?

In the first place, since F is transitive, there are as many substitutions in the sought group V that end in 8 as that end in any other element; and if we collect those terminating in 8, we shall have a group of $7\cdot 6\cdot 4$ substitutions, in which, if we erase the 8, we shall have a group of $7\cdot 6\cdot 4$ made with seven elements.

In the same way, if we collect in this group of $7\cdot 6\cdot 4$ the terms ending in 7, and erase the 7, we shall have a group J of twenty-four made with six letters.

There are several groups of twenty-four made with six elements. The required group J in this case is the following:

123456	435621	652134	
342165	126543	564321	(J)
214356	345612	651243	
431265	216534	563412	
124365	346521	562143	
432156	125634	653421	
213465	436512	561234	
341256	215643	654312,	

which is half the group of $48=(\pi 2)^3\pi 3$, of Art. 51, made with $6=2\cdot 3$ elements.

This is a modular group. If we multiply it, after the addition of 7 final throughout, by the seven powers of

$$\theta=7513426,$$

we obtain a non-modular group L of $7\cdot 6\cdot 4$ substitutions.

The existence of the group just written is easily proved by the test, putting J′ for J with 7 final,

$$\mathrm{L}=\mathrm{J}'(1+\theta+\theta^2+\cdot\cdot+\theta^6)=(1+\theta+\theta^2+\cdot\cdot\theta^6)\mathrm{J}'.$$

We shall return presently to the direct construction of these groups of $7\cdot 6\cdot 4$, which are of considerable interest, by reason of the learned and instructive researches of Galois, who first affirmed their existence, as well as that of the groups of $11\cdot 10\cdot 6$ made with eleven elements (*vide* Hermite's *Théorie des Équations modulaires*, § xiv.), and also by those of MM. Betti of Pisa, Kronecker of Berlin, and Hermite of Paris, who appear all to have constructed these groups.

Has anything more been done during the last twenty years in this theory of groups, apart from the recent competition, beyond the construction of these groups implicitly discovered by Galois, and Mr. Cayley's analysis of groups of the eighth order?

It suffices for my purpose here to shew, that a vast number of non-modular groups, and consequently often of modular groups made by adding to them their derived derangements, is given by the theorems demonstrated in the preceding Memoir.

85. To form the modular group of $8\cdot 7\cdot 6\cdot 4$, we add 8 final to every substitution of L, and then multiply L′ thus formed by the group F

12345678
21436587
34127856

$$\begin{matrix} 43218765 \\ 56781234 \\ 65872143 \\ 78563412 \\ 87654321 \end{matrix} \qquad (\mathrm{F})$$

one of the thirty equivalents formed on the partition

$$8=2\cdot4=\mathrm{A}a.$$

We thus form $7\cdot6\cdot4-1$ derived derangements of F. For example: the derangements of F by $\theta=75134268$ above written, by $a=43562178$ the second substitution of J′, and by $\theta a=31425768$ their product in L′, are

75134268	43562178	31425768
86243157	34651287	42316857
57312486	21784356	13247586
68421375	12873465	24138675
31578624	87126534	75861324
42687513	78215643	86752413
13756842	65348712	57683142
24865731	56437821	68574231;

and these are also derivates of F.

We have then the modular group

$$\mathrm{V}=\mathrm{F}\Theta\mathrm{J}'',$$

where J″ is the group J of twenty-four augmented throughout by 78 final.

Θ is the group of powers of

$$\theta=75134268,$$

and

$$\Theta\mathrm{J}''=\mathrm{L}'$$

is the group of $7\cdot6\cdot4$ substitutions having 8 final.

By the group L, omitting 8 final in L′, we can form functions of seven letters having thirty values, and by the group V we make functions of eight letters having thirty values, which are, so far as I know, new.

Our group

$$\mathrm{V}=\mathrm{F}\Theta\mathrm{J}''$$

consists of a group of $8\cdot 7$ substitutions $F\Theta$, multiplied by the group J''.

This group of fifty-six is the product of the group F by the powers of $\theta = 75134268$. The product

$$F\theta = \theta F$$

is written above. Every substitution m in it is of the form

$$\theta\lambda = \mu,$$

λ being a substitution of F, whence

$$F\mu = \mu F$$

is the same derivate of F. Also

$$\lambda\theta\lambda = \lambda\mu = \nu$$

is another substitution of this derivate. But this is

$$\nu = \lambda\theta\lambda^{-1} = \lambda\theta\lambda,$$

which is therefore, by a known theorem of Cauchy, of the same order with θ; that is, every substitution not in F, of the group of the order fifty-six

$$F\Theta,$$

is a substitution of the seventh order.

There are therefore $6\cdot 8 = 48$ substitutions of the seventh order in this group $F\Theta$ of fifty-six.

86. *The number of substitutions of any form*

$$N = Aa + Bb + \cdot\cdot$$

having a circular factors of the order A, b of the order B, &c., is given by the theorem C, (12), and I believe it to be given nowhere else. For if W be the number of equivalent groups there enumerated, $R_K W$ *is the number sought of substitutions.*

Now there are $8\cdot(\Pi 5)\cdot 6$ different substitutions of the seventh order made with eight letters; and every group of fifty-six equivalent to this $F\Theta$ contains forty-eight of them. It follows that there are not fewer than

$$\frac{6\cdot 8\cdot \Pi 5}{48} = \Pi 5$$

equivalent groups of fifty-six; for every substitution of the seventh order will appear in at least one group of

fifty-six. But there is nothing to prevent the number of equivalents from being such that every substitution θ' of the seventh order made with seven letters shall appear more than once among them. Let r be the number of such repetition of every θ'; then is $120r$ the number of the equivalent groups of fifty-six.

Each of these equivalents contains one of the thirty equivalents of F. Wherefore

$$\frac{120r}{30}=4r$$

is the number of different sets of forty-eight substitutions of the seventh order that may be so added to a given group F as to complete a group of fifty-six.

The value of r in this case is $r=2$; and, in fact, if we multiply F, our group of eight,

12345678
21436587
34127856
43218765
&c.,

by the powers of any one of the eight substitutions following,

24865731 λ_1
24861375 λ_2
24758613 λ_3
24753168 λ_4
24685713 λ_5
24683175 λ_6
24571368 λ_7
24578631 λ_8,

of which the first gives the derangement $F\theta=F\lambda$, above written, we shall form eight different groups of fifty-six on this group F.

There are, consequently, $8\cdot 30=240$ equivalent groups of fifty-six; whence by the theorem A, (9), there are 240

equivalent modular groups of

$$\frac{\pi\cdot 8}{240}=8\cdot 7\cdot 3$$

substitutions, made by adding to a group of fifty-six two derived derangements.

This group of $8\cdot 7\cdot 3$, like that of $8\cdot 7$, is subordinate to the maximum modular group V of $8\cdot 7\cdot 6\cdot 4$, and forms part of it. V can be written either as the group F with twenty derived derangements, or the group of fifty-six with two.

The group of $8\cdot 7$ is one of the groups included under M. Mathieu's theorem, that whenever N is a power of a prime number, there is a group of $N\cdot(N-1)$ substitutions.

All these groups of $N\cdot(N-1)$ are modular, and form subordinate portions of the maximum groups given by the theorems H (37) and A (9).

The theory of them all is like that of the above groups of $2^3\cdot(2^3-1)$; and the construction of these groups on any partition

$$N=n^i,$$

n being a prime number, is easy, as well as the enumeration of their equivalents, which, being known, the theorem A gives the maximum groups to which they belong.

87. *Non-modular groups of* $N\cdot\overline{N-1}\ \overline{N-2}$ *substitutions when* $N-1=p^i$, *p being any prime number.*

These groups were first discovered by M. Mathieu; but nothwithstanding the elegance of his demonstration, by the method of imaginary subindices, there are few readers who will not desire a mode of investigation somewhat less learned. The following is a sketch of a tactical demonstration of these groups, and is little else than a repetition of that given above for $i=1$.

The group (L) of $N-1$ substitutions, which is formed by the theorem H, (37), on the partition

$$N-1=p\cdot p^{(i-1)}=Aa,$$

is $(L)=1\lambda_1\lambda_2\lambda_3\cdot\cdot$ where $\lambda_1\lambda_2\cdot\cdot$ are all of the p^{th} order.

The substitutions

$$\mu_1\mu_2\mu_3\cdot\cdot\mu_{N-1}$$

of any of the derived derangements of (L) are such that we have always

$$\lambda_g\mu_a=\mu_c,\quad \mu_a\lambda_g=\mu_e,$$
$$\mu_a\lambda_g^{-1}=\mu_b,\ \lambda_g^{-1}\mu_a=\mu_a\,;$$

wherefore

$$\lambda_g\mu_a\lambda_g^{-1}=\mu_c\lambda_g^{-1}=\mu_f$$

is a substitution of the derivate, which is similar to μ_a; and the derivate is composed of $N-1$ similar substitutions.

The group of M. Mathieu,

$$K=LM_n,$$

of $(N-1)(N-2)$ substitutions made with the elements $123\cdot\cdot(N-1)$, which we suppose to be given, is

$$K=L+\mu_nL+\mu_n^2L+\mu_n^3L+\cdot\cdot\mu_n^{N-2}L,$$

which can be written also as the product

$$K=L\cdot M_1\cdot M_2\cdot M_3\cdot\cdot M_{N-1},$$

where M_d is the group of $(N-2)$ powers of μ_d.

The derivate $\mu_n^{\frac{N-2}{2}}$ L, when $\dfrac{N-2}{2}$ is integer, that is, when $p>2$, is always a system of didymous radicals of $\lambda_1\lambda_2\lambda_3\cdot\cdot\cdot$

Let the $N-2$ didymous radicals of M_r be written below M_r. We have now $N-1$ model groups

$$M'_1M'_2M'_3\cdot\cdot M'_{N-1}$$

each of the order $2(N-2)$, and each one having a different element undisturbed in a vertical row.

Let N final be added to every substitution of K, and in M'_r so augmented let N be exchanged in all the $N-2$ didymous radicals with the undisturbed letter r. The result is M''_r which is still a group of $N-2$ powers with $N-2$ didymous radicals.

The other groups of the order $2(N-2)$ are

$$\lambda_1 M''_r \lambda_1^{-1},\ \lambda_2 M''_r \lambda_2^{-1},\ \lambda_3 M''_r \lambda_3^{-1},\ \&c.;$$

whence we see that if a be any one of the $(N-1)(N-2)$ didymous radicals added to K,

$$\lambda_m a \lambda_m^{-1} = \beta$$

is another, and we have

$$\lambda_m a = \beta \lambda_m,$$

whichever λ_m may be of the substitutions $\lambda_1 \lambda_2 \lambda_3 \cdots$

Let $a_n b_n c_n \cdot\cdot$ be the didymous radicals of the group M''_n. If we write all the derivates

$$a_n L = a_n + a_n\lambda_1 + a_n\lambda_2 + \cdot\cdot$$
$$b_n L = b_n + b_n\lambda_1 + b_n\lambda_2 + \cdot\cdot$$
$$c_n L = c_n + c_n\lambda_1 + c_n\lambda_2 + \cdot\cdot$$
$$\vdots$$

we have thus formed

$$a_n K = a_n L + a_n\mu_n L + a_n\mu_n^2 L + \cdot\cdot$$

since

$$a_n\mu_n = b_n$$
$$a_n\mu_n^2 = c_n,\ \&c.$$

This derivate a_nK will have for its final vertical row the letter n which is undisturbed in M''_n. We can thus complete $N-1$ derivates of K, which along with K make

$$(N-1)(N-2)+(N-1)(N-1)(N-2) = N(N-1)(N-2)$$

substitutions, to every one of which (a) corresponds another (β) such that

$$\lambda_m a = \beta \lambda_m.$$

88. It can be proved by a simple tactical argument similar to that mentioned in Art. 81, that if a and β be any two of the square roots of unity of the system of groups of the order $2(N-2)$,

$$M''_1\ M''_2 \cdot\cdot,$$

(which radicals, including $\mu_1^{\frac{1}{2}(N-2)}$, $\mu_2^{\frac{1}{2}(N-2)}$, &c., when $N-2$ is even, that is, when $p>2$, *which we suppose to be the case,* make up the number

$$(N-1)(N-1)+N-1=(N-1)^2),$$

$\beta a\beta=\gamma$ is another.

Now if we write under $\mu_1^{\frac{1}{2}(N-2)}=a_1$, which has two letters undisturbed in M''_1 those didymous radicals of any other group M''_n, which have no letter undisturbed, we always find a given number of them $\beta'\beta''\cdot\cdot$ such that

$$\frac{a_1}{\beta'}=a_1\beta'=\phi$$

is a substitution ϕ of the N^{th} order. Wherefore $\beta' a_1\beta'=\gamma$, $\gamma\beta\gamma=\delta$, $\delta\gamma\delta=\epsilon$, &c., the whole series of didymous radicals of ϕ, $\phi^2\cdot\cdot$, are in the system of $(N-1)^2$ radicals. As no two of these can terminate with the same letter, there is one of them, not a_1, in each of the $N-1$ derivates of K that we have formed, which are therefore the derivates

$$\beta'K \quad \gamma K \quad \delta K, \text{ \&c.}$$

Now

$$\beta'K \text{ contains } \beta' a_1=\phi^{-1},$$
$$\gamma K \text{ contains } \gamma a_1=\phi^{-2},$$
$$\text{\&c.};$$

that is, our $N-1$ derivates are

$$\phi K+\phi^2K+\cdot\cdot+\phi^{N-1}K.$$

Thus we complete the demonstration, which is to be slightly modified if $p=2$, that we have formed a group of $N\cdot(N-1)(N-2)$ substitutions; *and these are the groups given by the important theorem of M. Mathieu, vide* Liouville's *Journal*, January, 1860.

As a complete demonstration of these groups, when $N-1$ is a prime number, has been given in § 5 of the preceding Memoir, and as the elegant demonstration of the more general theorem is easily accessible in the *Journal* just referred to, it is hoped that the readers to whom the subject is interesting will pardon the brevity with which, under restrictions as to space, I have laid before them the *tactical investigation*, to which I do not presume to attach more than a secondary importance.

89. *The group of* $N\cdot(N-1)(N-2)$, *when* $N-1$ *is a prime number, is always a maximum.* It has no derived derangement.

The group of $N\cdot(N-1)(N-3)$, *when* $N-1$ *is a power greater than the first of a prime number, is never maximum.* It has always derived derangements, which can be enumerated by the application of the theorems of this Memoir.

Thus when

$$N-1=2^3=8,$$

there are, as has been shewn above, 240 equivalent groups of $8\cdot7$ made with eight elements, and consequently 240 groups of fifty-six made with nine elements, in which the ninth element is final and undisturbed. Each of these 240 determines a group of $9\cdot8\cdot7$. We have then 240 equivalent groups of $9\cdot8\cdot7$; wherefore by my theorem A there are 240 equivalent groups each of

$$\frac{\pi 9}{2411}=9\cdot8\cdot7\cdot3$$

substitutions, of which each one contains one of M. Mathieu's groups of $9\cdot8\cdot7$.

If in this group of $9\cdot8\cdot7\cdot3$ we collect the substitutions terminating with 9, we shall, after erasing 9, have the group of $8\cdot7\cdot3$ which has been found before, (86), and which is made by adding to the group of $8\cdot7$ two derived derangements of it.

The number of equivalent groups of nine containing each eight substitutions of the third order is given by the theorem H to be 840. There are, consequently, by the theorem A 840 equivalent modular groups each of

$$\frac{\pi 9}{840}=9\cdot8\cdot6$$

substitutions.

M. Mathieu's group of $3^2\cdot(3^2-1)$ is part of this maximum group of $9\cdot8\cdot6$, and is given with it. Each of these

groups of seventy-two can be completed in three ways, and in three ways only, into one of M. Mathieu's groups made with ten elements of

$$(3^2+1)3^2\cdot(3^2-1)$$

substitutions.

Then by the theorem A there are $3\cdot840$ equivalent groups made with ten elements each of

$$\frac{\pi10}{3\cdot840}=10\cdot9\cdot8\cdot2,$$

and comprising substitutions of the tenth order.

M. Mathieu's group of $10\cdot9\cdot8$ is a portion of this maximum group of $10\cdot9\cdot8\cdot2$.

If in this group we collect the terms ending with 10, and erase 10, we have a group of $9\cdot8\cdot2$ made with nine elements. This is a portion of the maximum group of $9\cdot8\cdot6$, above found, and contains the group of $9\cdot8$ found by M. Mathieu.

What is above extracted from the theorems of the preceding Memoir is merely an example of what they will yield for any partition $N-1=n^i$, n being any prime.

90. *Groups of* $\frac{1}{2}\overline{N+1}\,N\cdot N-1$ *made with* N *elements.*

Let N be any prime number.

Let β be any primitive root of the congruence

$$x^{\frac{1}{2}(N-1)}-1\equiv0 \ (\text{mod. } N).$$

There is a group G of $N\,\dfrac{N-1}{2}$ substitutions all of the form

$$\beta^r i+c, \ (r\overline{>}0),$$

where c has any of the values $012\cdot\cdot N-1$. This group

$$G=S(\beta^r i+c)$$

can be written as the product of $g=S(i+c)$ of the N^{th} order, and N groups each of the order $\frac{1}{2}(N-1)$, $h_1, h_2, h_3\cdot\cdot h_N$, which all contain, besides unity, only substitutions of

the $\frac{1}{2}(N-1)^{th}$ order formed on the partition

$$N=\left(\tfrac{1}{2}\overline{N-1}\right)2+1\cdot 1=Aa+Bb.$$

To each of these can be added $\frac{1}{2}(N-1)$ didymous radicals, and this can be done in $\frac{1}{2}(N-1)$ different ways, by theorem G, (26); $\frac{1}{2}(N-1)=\frac{1}{k}A^a$, (25).

One of the groups (h), h_N, which has N final undisturbed, is composed of $\frac{1}{2}(N-1)$ substitutions

$$\beta^0 i,\ \beta i,\ \beta^2 i,\cdot\cdot\beta^{\frac{1}{2}(N-3)}i;$$

and any one of its $\dfrac{N-1}{2}$ systems of didymous radicals is

$$\phi i,\ \phi(\beta i),\ \phi(\beta^2 i)\cdot\cdot\phi\beta^{\frac{1}{2}(N-3)}i,$$

where

$$\phi i(=)i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)})\ \ (\text{mod. N})$$

if A be determined by the condition

$$\phi i=c,\ \ (\text{mod. N}),$$

c being any number $<$N which is no power of β.

91. It is only necessary to prove here that

$$(\phi i)^2(=)i,$$

or that ϕi is a square root of unity, whence it readily follows that

$$\phi(\beta^m i)\phi(\beta^n i)(=)\beta^p i,\ \ (\text{mod. N}),$$

one of the substitutions of h_N.

The former of these equations affirms the congruence

$$i\equiv(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))^{-1}+A(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))^{-1}$$
$$-A(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))^{\frac{1}{2}(N-3)};$$

or that

$$i(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))\equiv 1+A-A(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))^{\frac{1}{2}(N-1)},$$

which is

$$1+A-Ai^{\frac{1}{2}(N-1)}\equiv 1+A-A(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))^{\frac{1}{2}(N-1)}.$$

This is true on condition that

$$i^{\frac{1}{2}(N-1)}\equiv(i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)}))^{\frac{1}{2}(N-1)}\equiv(\phi i)^{\frac{1}{2}(N-1)},\ \ (\text{mod. N}).$$

Now whatever element $< N \ \phi i$ may be, the only values that its $\frac{N-1}{2}^{th}$ power can have, to modulus N, are ± 1.

When $i^{\frac{1}{2}(N-1)}=1$, that is, when i is a power of β, or when

$$i^{-1}=i^{\frac{1}{2}(N-3)},$$

we have

$$(i^x)^{\frac{1}{2}(N-1)}=1,$$

whatever x may be, and therefore

$$(\phi i)^{\frac{1}{2}(N-1)}=(i^{-1})^{\frac{1}{2}(N-1)}=1.$$

All that is necessary in order that

$$(\phi i)^{\frac{1}{2}(N-1)}=-1=i^{\frac{1}{2}(N-1)}, \text{ and } (\phi i)^2=i,$$

when i is no power of β, *is that ϕi shall be no power of β.* We may thus determine ϕi by the condition

$$\phi c=c,$$

c being any number which is no power of β. And thus we obtain all the $\frac{N-1}{2}$ systems of didymous radicals, by using for the determination of A in ϕi, $\frac{N-1}{2}$ different values of c, no power of β.

92. Let h_e and any of its systems of didymous radicals form the group H_e of the $(N-1)^{th}$ order. The remaining $N-1$ groups

$$h_1 h_2 h_3 \cdot\cdot$$

of the order of h_e are all of the form

$$(i+k)h_e(i-k)=\theta^k\cdot h_e\theta^{-k};$$

and we have as many groups $H_1H_2H_3\cdot\cdot$ of the $(N-1)^{th}$ order by giving to k the values $123\cdot\cdot(N-1)$ in

$$(i+k)H_e(i-k).$$

We thus add to the group

$$G=S(\beta i+c)$$

of the $N\cdot\frac{(N-1)^{th}}{2}$ order, by completing the N groups H_e, $N\cdot\frac{N-1}{2}$ didymous radicals of the form

$$(i+k)\phi\beta^n i(i-k),$$

which is

$$\phi' = B\beta^n(i-k)^{-1} - C\beta^n(i-k)^{\frac{1}{2}(N-3)} + k,$$

where B and C are given functions of the number c chosen. And it is evident that if $\phi_{,}$ be any one of the entire system of $N\cdot\frac{N-1}{2}$ radicals,

$$\theta^a\phi_{,}\theta^{-a} = \phi_{,,} \quad (\theta = i+1)$$

is another, and that

$$\theta^a\,\phi_{,} = \phi_{,,}\,\theta^a.$$

Add now, to the $N\frac{N-1}{2} + N\cdot\frac{N-1}{2}$ substitutions in G and the added didymous radicals, $N-1$ cyclical permutations

$$\phi\theta,\ \phi\theta^2\cdot\cdot,\ \phi\theta^{N-1}$$

of every radical ϕ. It is plain that we have thereby added $(N-1)N\cdot\frac{N-1}{2}$ substitutions

$$\theta\phi_{,}\ \ \theta^2\phi_{,,}\ \ \theta^2\phi_{,,,},\ \&c.$$

All that is further required, in order that the $\overline{N+1}$ $N\cdot\frac{N-1}{2}$ substitutions thus formed should be a group, is, that the product $\phi_{,}\phi_{,,}$ of any two of the $N\cdot\frac{N-1}{2}$ didymous radicals should be either another or a cyclical permutation of another; that is, we must have

$$\phi_{,}\phi_{,,} = \phi''\theta^a = \theta^b\phi'' = \phi'' + b.$$

There is no difficulty in ascertaining this point algebraically, except the usual one of elimination.

The condition to be satisfied is

$$\begin{aligned} &\{B(\beta^n(i-k))^{N-2} - C(\beta^n(i-k))^{\frac{1}{2}(N-3)} + k\} \\ \times\ &\{B(\beta^m(i-h))^{N-2} - C(\beta^m(i-h))^{\frac{1}{2}(N-3)} + h\} \\ (=)\ &\{B(\beta^x(i-y))^{N-2} - C(\beta(^x(i-y))^{\frac{1}{2}(N-3)} + z\} \qquad (Q), \end{aligned}$$

and it is required, in order that the group exist, that x, y, z be determined in terms of $BCkhmn$, and that c, of which B and C are given functions, should be found by an equation independent of $mnkh$.

93. The only important point is to ascertain the result-

ing equation in c; for c being found, the group is easily constructed.

Let

$$J=B(\beta^m(i-h))^{N-2}-C(\beta^m(i-h))^{\frac{1}{2}(N-3)}+h.$$

The tactical equation Q becomes the congruence

$$B(\beta^n(J-k))^{N-2}-C(\beta^n(J-k))^{\frac{1}{2}(N-3)}+k$$
$$\equiv B(\beta^x(i-y))^{N-2}-C(\beta^x(i-y))^{\frac{1}{2}(N-3)}+z,$$

the modulus being N.

The highest power of i on either side of this congruence is i^{N-2}, since, whatever i may be,

$$i^{N-1}\equiv 1 \pmod{N}.$$

By equating the coefficients of the $N-1$ powers of i, we can eliminate linearly the $N-1$ variables

$$z\ y\ y^2 y^3\cdot\cdot y^{N-2};$$

and we obtain an equation

$$V\equiv 0,$$

containing only the $\frac{N-3}{2}$ powers >0 of β^x.

Then by adding to $V\equiv 0$ the $\frac{N-3}{2}$ equations

$$\beta^x V\equiv 0,\quad \beta^{2x}V\equiv 0,\quad \&c.,$$

which will introduce no higher powers of β^x, as $\beta^{\frac{1}{2}(N-1)}\equiv 1$, we can eliminate linearly all these powers.

If the group has any existence, we shall obtain a result containing BC free from $mnkh$, which will be a congruence

$$Fc\equiv 0 \pmod{N}.$$

The integer solutions of this, which are not powers of β, give each a distinct set of $N\cdot\frac{N-1}{2}$ didymous radicals, which sets, with their cyclical permutations, will complete the group

$$G=S(\beta i+c)$$

into as many distinct groups of $\frac{1}{2}(N+1)N\cdot(N-1)$ substitutions.

We readily find the resulting quadratic for $N=7$, giving

$c=3$ and $c=5$. And it is easy to satisfy one's self that the equation Q is satisfied when $N=11$, by taking $c=7$ or $c=8$ for the determination of A in ϕi; that is, Q is satisfied when $N=7$ by

$$B=-1 \text{ and } C=-2,$$

or by

$$B=-2 \text{ and } C=-3,$$

and when $N=11$ it is satisfied by

$$B=5 \text{ and } C=4,$$

or by

$$B=3 \text{ and } C=2.$$

94. But there is no need to attempt the eliminations to which the condition Q invites us. We can much more readily settle the matter by tactical considerations; that is, we can determine whether a given value of c in

$$\phi i=i^{-1}+A(i^{-1}-i^{\frac{1}{2}(N-3)})=Bi^{-1}-Ci^{\frac{1}{2}(N-3)}$$

gives one of a system of didymous radicals which will complete the group in question.

If the equation Q be satisfied by the system, it is satisfied when $\beta^m=\beta^n=\beta^0=1$. We must have, putting $k=0$, and $\beta^m=1=\beta^n$,

$$\phi i(i+h)\phi i(i-h)(=)(i+z)\phi\beta^x(i-y),$$

whence comes

$$\phi i(i+h)\ \phi i(=)(i+z)\ \phi(\beta^x(i-y))(i+h)(=)(i+z)\ \phi(\beta^x(i-t)).$$

As there are N values of h and only $\frac{N-1}{2}$ of x, there will be one or more values of h that will introduce $\beta^x=\beta^0=1$ into the right member. If then the group exists,

$$\psi i=\phi i(i+h)\ \phi i(=)(i+z)\ \phi(i-t)(=)\ \phi(i-t)+z=\lambda^h.$$

This affirms that ψi, the h^{th} power of the substitution of the N^{th} order, λ, which is determined by the circular factor ϕi, differs from a certain cyclical permutation $\phi(i-t)$ of ϕi, only by the addition of the constant z to every element; that is, if

$$\phi i=abcde\cdot\cdot,$$

$$\psi i=a'b'c'd'e'\cdot\cdot,$$

the circle of differences

$$b-a,\ c-b,\ d-c,\ e-d\cdots$$

is exactly the circle of differences

$$b'-a',\ c'-b',\ d'-c',\ e'-d'\cdots$$

begun at some other point than $b'-a'$.

We have only to form the N powers of the substitution λ, by writing under 1 in unity the vertical circle ϕi, of which the first element is always 1, whatever c may be, and then completing the same vertical circle under every element of unity.

If there be no power of λ whose circle of differences is that of ϕi, the group sought has no existence.

If there be such power or powers, we have found ϕi consistent with equation Q, and as all the unknowns of Q in the right member can be determined in terms of B and C, that is of A in ϕi, we have proof that the group exists, for the system of didymous radicals given with ϕi.

95. It is easy to prove by this method that no such group of $\frac{1}{2}(N+1)N\cdot(N-1)$ exists for $N=13$ or $N=17$ or $N=19$.

The substitutions ϕi which give the sought groups are for $N=7$,

$$\phi i=-i^{-1}+2i^2=1462537,$$

and

$$\phi i=5i^{-1}+3i^2=1432657;$$

and for $N=11$,

$$\phi i=3i^{-1}-2i^4=1843907256a,$$

and

$$\phi i=5i^{-1}-4i^4=1043976852a.$$

The four groups thus found of the order $\frac{1}{2}(N+1)N\cdot(N-1)$ may be thus expressed as products, denoting by $\{H\}_m$ the group of powers of the substitution H of the m^{th} order.

$$\{2345671\}_7\{1357246\}_3\{1426735\}_4\{1462537\}_2=F$$
$$\{2345671\}_7\{1357246\}_3\{1347652\}_4\{1432657\}_2=F'$$

$$\{234567890a1\}_{11}\{3691470258a\}_5\{3819507246a\}_4$$
$$\times\{6518934720a\}_3=\mathrm{E}$$
$$\{234567890a1\}_{11}\{3691470258a\}_5\{2407185936a\}_4$$
$$\times\{5629834170a\}_3=\mathrm{E}_1.$$

The three last factors of F or of F_1 are a modular group (9) of twenty-four made with six elements (234567).

The three last factors of E or of E_1 are a non-modular group of sixty made with (1234567890). The last two factors of any of the four groups are modular groups also, of eight or of twelve, given by the application of the preceding theorems.

All these groups of $8\cdot 7\cdot 3$ and $12\cdot 11\cdot 5$ are maximum and non-modular, whose equivalents can be easily enumerated, as can those of the inferior groups exhibited.

The group F is formed by adding to $\mathrm{G}=\mathrm{S}(2^r i+c)$, of the twenty-first order, $7\cdot 7\cdot 3$ substitutions of the form

$$\chi i=6(2^r\overline{i-a})^{-1}+2(2^r\overline{i-a})^2+b.$$

The group F′ is formed by adding to the same G, $7\cdot 7\cdot 3$ substitutions of the form

$$\chi i=5(2^r\overline{i-a})^{-1}+3(2^r\overline{i-a})^2+b,$$

where a or b may have any of seven values 0123456.

The group E is formed by adding to $\mathrm{G}'=\mathrm{S}(3^r\cdot i+c)$ of the fifty-fifth order, $11\cdot 11\cdot 5$ substitutions of the form

$$\chi i=5(3^r(i-a))^{-1}-4(3^r(i-a))^4+b;$$

and E_1 is made by adding to the same G′ as many of the form

$$\chi i=3\cdot(3^r(\overline{i-a}))^{-1}-2\cdot(3^r(\overline{i-a}))^4+b,$$

where a and b have each any of eleven values.

The groups F and E_1 are those given by M. Hermite at page 63 of his *Théorie des Équations modulaires*, Paris, 1859. It may not have been observed before, that there is one, and one only, of the equivalents of F or of E which has for a factor the same cyclical group of $7\cdot 3$ or of $11\cdot 5$ substitutions. The functions constructed on F or E will have no value in common with those given by F_1 or E_1.

96. There does not appear to be any general theorem on groups of the order $\frac{1}{2}(N+1)N\cdot(N-1)$ when N is prime, to which the groups above found are to be referred.

The true generalization of these theorems will be found in the *tactical path* which has so readily conducted us to the group of $7\cdot6\cdot4$ and to the higher group of $8\cdot7\cdot6\cdot4$ of which, when augmented by 8 final, it is a factor, viz. by applying the theorem H (37) and the corollary of theorem A (9) to partitions of the form

$$N=Aa+Bb+\cdots\quad=A\cdot Aa_1+B\cdot Bb_1+\cdots$$

Thus we easily prove that there are, taking $w=30$ for the number of auxiliary groups of eight,

$$S=\frac{1\cdot1\cdot(15\cdot13\cdot11\cdot9\cdot7\cdot5\cdot3)\cdot30\cdot2^{8-4}}{2(7\cdot2+(8-7)1)}=15\cdot14\cdot13\cdot11\cdot9\cdot6\cdot2$$

equivalent groups of sixteen, containing each fifteen principal substitutions of the second order; whence by the corollary (9) *there are* S *maximum equivalent modular groups of* $16\cdot15\cdot12\cdot8\cdot7\cdot4$, *made with sixteen elements*, which may be treated as we treated the group of $8\cdot7\cdot6\cdot4$ in Art. (84).

97. *On grouped groups, and the forms of the operative or auxiliary groups.*

In the preceding Memoir there are (38, 39) examples of the mode of operation on a model group, that is, any group containing unity, by an auxiliary group, whose elements represent each one an elementary group of the model group. We thus produce a vast number of modular groups, by adding to a model a defined series of derived derangements, which I believe cannot be found by any method elsewhere directly indicated.

What follows is a brief account of my continuation of the investigation there opened.

The exponents of the circular factors described (37, 39) in theorem H are such that, by their variation, only

groups are obtained equivalent to that given when all the exponents are unity.

Thus, for example, if we operate on the model

$$\begin{matrix} 123456789 \\ 231564897 \\ 312645937 \end{matrix} \quad \text{(H)}$$

by the auxiliary

$$\begin{matrix} 1\,2^3 3^2 \\ 2^3 3^2 1 \\ 3^2 1\,2^3, \end{matrix} \quad (k)$$

which adds to (H) its derivates by

$$Q_1 = \frac{645 \cdot 897 \cdot 123}{123 \cdot 645 \cdot 897} = \frac{2^3 3^2 1}{1\,2^3 3^2}$$

and by

$$Q_2 = \frac{897 \cdot 123 \cdot 645}{123 \cdot 645 \cdot 897} = \frac{3^2 1\,2^3}{1\,2^3 3^2},$$

we form a group of nine, equivalent to that obtained by operation with the auxiliary

$$\begin{matrix} 123 \\ 231 \\ 312. \end{matrix} \quad (k_0)$$

But there is *a curious extension of this theory, whereby we obtain groups of nine not so equivalent.*

Let the operative group be

$$\begin{matrix} 1\,2\,3 \\ 2^2 3\,1 \\ 3^2 1\,2^2 \end{matrix} \quad (k_2)$$

with the definitions, $2^4=2$, $3^4=3$, $1^4=1$.

We find that (k_2) is a group by the test

$$2^2 31 \cdot 3^2 12^2 = 1^2 2^2 3^2 = 3^2 12^2 \cdot 2^2 31\,;$$

for first, the operation $2^2 31$ on the subject $3^2 12^3$ puts 2^2 for 1, 3 for 2, that is 3^2 for 2^2, and 1 for 3, that is 1^2 for 3^2; and the operation $3^2 12^2$ on the subject $2^2 31$ puts 3^2 for 1, 1 for 2, that is 1^2 for 2^2, and 2^2 for 3; and secondly,

$$1^a 2^a 3^a = 123$$

is the model, whatever a may be, written in a different

order: that is,

$$\begin{array}{l} 1^2 2^2 3^2 = 231564897 \\ 312645978 \\ 123456789. \end{array}$$

The result of operation with (k_2) is to add to the model H the two derivates

$$\begin{array}{l} 564789123 \\ 645897231 \\ 456978312 \\ \\ 897123564 \\ 978231645 \\ 789312456, \end{array}$$

which complete a group of nine containing three cube roots of 231564897, and three cube roots of 312645987, all six of the ninth order.

If we operate with each of

$$\begin{array}{lllll} 1\ 2\ 3 & 1\ 2\ 3 & 1\ 2\ 3 & 1\ 2\ 3 & 1\ 2\ 3 \\ 2\ 3^2 1 & 2\ 3\ 1^2 & 2^3 3\ 1 & 2\ 3^3 1 & 2\ 3\ 1^3 \\ 3^2 1^2 2, & 3\ 1^2 2^2, & 3^3 1\ 2^3, & 3^3 1^3 2, & 3\ 1^3 2^3, \end{array}$$

which are all alike groups, we add to H five more sets of six substitutions of the ninth order; *and we thus solve the curious problem in evolution, to find the eighteen cube roots of* 231564897 *and the eighteen cube roots of* 312645978.

It will be found impossible to modify the vertical rows of these operative groups, as we can those of Art. 39, so that the same element shall everywhere shew the same exponent.

98. Let the model be

$$\left.\begin{array}{l} 12345678 \\ 21436587 \end{array}\right. \quad \mathrm{K}.$$

and the auxiliary

$$\begin{array}{l} 1\ 2\ 3\ 4 \\ 2^2 1\ 4\ 3^2 \\ 3^2 4^2 1\ 2 \\ 4\ 3^2 2\ 1^2. \end{array} \quad (k')$$

This (k') is a group, as appears by the tests,

$$2^2143^2(k')=(k')2^2143^2, \ \&c.$$

We have in the case of this model, if $1=\frac{12}{21}$, $2=\frac{34}{43}$, &c,

$$1^3=1, \ 2^3=2, \ 3^3=3, \ 4^3=4.$$

The result of operation by (k') on K is

12345678
21436587
43127865
34218756
65871234
56782143
78653421
87564312,

a group first divined and defined by Mr. Cayley in an elegant little paper in the *Philosophical Magazine* for 1859. The difficulty of the step from the analytical definition of a group to its actual construction, is shewn by the fact, that Mr. Cayley did not succeed in constructing this group till long after he had published its definition.

I hope that one great use of this Memoir will be to facilitate the tactical construction of groups, as well as the enumeration of their equivalents. Hereby the *fonctions bien définies* (68) of the Paris Prize Question for 1860 will be accurately formed and exhausted.

This group of Mr. Cayley's has six substitutions of the fourth order all square roots of 21436587.

If we operate on the same model with

$$\begin{matrix} 1 & 2 & 3 & 4 \\ 2^2 1 & 4^2 3 \\ 3^2 4 & 1 & 2^2 \\ 4^2 3^2 2 & 1, \end{matrix} \qquad (k'')$$

we obtain the group

12345678
21436587
43128756

34217865
65781243
56872134
87653412
78564321

equivalent to the preceding, which has six more square roots of 21436587.

What has just been done is a case of a more general theorem on grouped groups. The two following

$$\begin{array}{llll} 1 & 2 & 3 & 4 \\ 2^d & 1 & 4 & 3^d \\ 3^d & 4^d & 1 & 2 \\ 4 & 3^d & 2 & 1^d, \end{array} \qquad \begin{array}{llll} 1 & 2 & 3 & 4 \\ 2^d & 1 & 4^d & 3 \\ 3^d & 4 & 1 & 2^d \\ 4^d & 3^d & 2 & 1, \end{array}$$

are true groups, if we define that

$$1^{2d-1}=1,\ 2^{2d-1}=2,\ 3^{2d-1}=3,\ 4^{2d-1}=4,$$

and that no substitution is changed in value by having its four exponents increased each by the same number. For example:

$$3^d4^d12\cdot 43^d21^d=21^d4^d3^{2d-1}=21^d4^d3=2^d143^d.$$

We may write in either of these groups, for v, any square group u of powers of a substitution of the $(2d-2)^{th}$ order made with $2d-2$ elements, v being each one of the four elements 1234, and for v^d the result of $d-1$ cyclical permutations of the vertical rows of u.

For example: if $d=4$, we have by the latter of the two auxiliaries the group following:

123456 7890*ab* *cdefgh* *ijklmn*
234561 890*ab*7 *defghc* *jklmni*
345612 90*ab*78 *efghcd* *klmnij*
456123 0*ab*789 *fghcde* *lmnijk*
561234 *ab*7890 *ghcdef* *mnijkl*
612345 *b*7890*a* *hgcdef* *nijklm*

0*ab*789 123456 *lmnijk* *cdefgh*
*ab*7890 234561 *mnijkl* *defghc*

*b*7890*a* 345612 *nijklm* *efghcd*
7890*ab* 456123 *ijklmn* *fghcde*
890*ab*7 561234 *jklmni* *ghcdef*
90*ab*78 612345 *klmnij* *hcdefg*

fghcde *ijklmn* 123456 0*ab*789
ghcdef *jklmni* 234561 *ab*7890
hcdefg *klmnij* 345612 *b*7890*a*
cdefgh *lmnijk* 456123 7890*ab*
defghc *mnijkl* 561234 890*ab*7
efghcd *nijklm* 612345 90*ab*78

lmnijk *fghcde* 7890*ab* 123456
mnijkl *ghcdef* 890*ab*7 234561
nijklm *hcdefg* 90*ab*78 345612
ijklmn *cdefgh* 0*ab*789 456123
jklmni *defghc* *ab*7890 561234
klmnij *efghcd* *b*7890*a* 612345.

This group contains only one square root of unity,

$$\theta = 4561230ab789fghcdelmnijk,$$

with two of its cube roots of the sixth order, six of its square roots of the fourth order, and twelve of its sixth roots of the twelfth order, besides two substitutions of the third order.

Or we may consider the above group of twenty-four as constructed by adding to the model group of the sixth order, which begins it, its three derived derangements by

$$\frac{2^4 1 4 3^4}{1234}, \quad \frac{3^4 4^4 1 2}{1234} \quad \text{and} \quad \frac{4 3^4 2 1^4}{1234},$$

where 1, 2, 3, 4, like $p_1 p_2 \cdot \cdot$ in Art. 27, stand for the four circular factors of the model group. And we may, by this method, easily construct an equivalent group of twenty-four on any model group equivalent to the one above employed, whether its circular factors, as above, are, or whether they are not, composed of contiguous elements of unity.

I hope soon to have the honour of presenting to the LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER a second Memoir, in which this Theory of Grouped Groups will be discussed in detail.

Here I shall merely remark, that it will often be found necessary, in handling the theorem H, (37), to include in the number w of equivalent auxiliary groups certain of those above described, which are not identical, at least as operators, with groups of the ordinary form, in which the same element can, by modification of vertical rows, be made to shew the same exponent, (39), whether unity or some other, wherever it appears.

Note on Art. 39, *page* 321, *of the Memoir on the Theory of Groups.*

I have neglected to observe, that if the group (g') of page 319 be transformed, with the understanding that the square groups of the fourth order, written at page 321, are to be substituted for the elements, the transformed group (g'') will be 123, $23^2 1^4$, $3^2 12^4$, $13^2 2^4$, $3^2 21^4$, 213, which differs from (g') by the addition of 3 to the last exponent of each triplet, the definitions being of course in this case $1^5 = 1$, $2^5 = 2$, $3^5 = 3$. The two last written triplets of page 321 should be $3^3 21^2$ and $2^2 1^3 3$. The reduction of exponents named in the fourth line of page 322 proceeds by the addition of the same number to every exponent in a vertical row of the auxiliary, the definitions being $1^{t+1} = 1$, $2^{t+1} = 2$, &c., if t be the number of vertical rows in the square groups to be substituted.

T. P. K.

CORRIGENDA.

Page 279, line 2, *for* be G, *read* be in G.
" 280, " 17, *for* AP, *read* A.
" 287, " 9, complete the line thus: "formed on the model G."
" 287, } *for* correction of Art. 14 and Art. 17, *see* Art. 75.
" 289, }
" 288, line 22, }
" 289, " 23 and 29, } *for* prime, *read* primitive.
" 293, " 16, }
" 300, " 25, *for* occupy, *read* occupy in the same order.
" 302, " 21, *for* $Be = Ce$, *read* $Be_1 = Ce_2$.
" 310, " 19, *for* lk, *read* λk.
" 316, " 8, *for* K, *read* k.
" 316, " 11, *for* $K(\lambda K + (l-\lambda)R_k$, *read* $k(\lambda k + (l-\lambda)R_k)$.
" 378, " 8, *for* V can be, *read* It can be.
" 379, " 10, *for* μ_a, *read* μ_d.
" 392, " 6, *for* 37, *read* 78.

XXIII.—*Remarks on the Theory of Rain.*
By Mr. Joseph Baxendell, F.R.A.S.

Read March 29th, 1860.

It has been well established by numerous carefully conducted experiments, that the quantity of rain received by a gauge placed on or near the ground is almost invariably greater than that received by a similar gauge placed at a greater elevation in the immediate neighbourhood; and, in explanation of this remarkable fact, meteorological writers have generally adopted the hypothesis advanced by Professor Phillips (*Report of the British Association for* 1833, p. 410), "that the whole difference in the quantity of rain at different heights above the surface of the neighbouring ground is caused by the continual augmentation of each drop of rain, from the commencement to the end of its descent, as it traverses successively the humid strata of air at a temperature so much lower than that of the surrounding medium, as to cause the deposition of moisture upon its surface." In support of this hypothesis, Professor Phillips remarks "that it takes account of the length of descent, because in passing through more air more moisture would be gathered; it agrees with the fact that the augmentation for given lengths of descent is greatest in the most humid seasons of the year; it accounts to us for the greater absolute size of rain-drops in the hottest months and near the ground, as compared with those in the winter and on mountains; finally, it is almost an

inevitable consequence, from what is known of the gradation of temperature in the atmosphere, that some effect of this kind must necessarily take place."

Now, although it must be admitted that the temperature of falling rain is generally below that of the air near the ground, yet if we proceed to determine the temperature of a rain-drop at the commencement of its descent, from its known rate of augmentation in falling, and from its temperature when it arrives at the surface of the ground, we shall obtain a result wholly inconsistent with known facts, and therefore fatal to the hypothesis.

The most complete series of observations with which I am acquainted, of the quantities of rain falling at different heights, is that made at York, in the years 1832-5, by Mr. Gray and Professor Phillips at the request of the British Association, the results of which are given and ably discussed by Professor Phillips in the volume of *Transactions of the Association for the year* 1835. Three gauges were used in these observations: the first was placed on a large grass-plot in the grounds of the Yorkshire Museum; the second on the roof of the Museum, at an elevation of 43 feet 8 inches; and the third on a pole 9 feet above the level of the battlements of the great tower of the Minster, at an elevation above the ground gauge of 212 feet 10½ inches. The total quantities of rain received by these gauges during the three years of observation were as follows:

1st gauge....	65·430	inches.
2nd „	52·169	„
3rd „	38·972	„

From these numbers it appears that the ratio of increase of size of a rain-drop is 0·679 for the last 213 feet of its fall, and 0·254 for the last 44 feet.

A very able discussion of the whole series of observa-

tions, with reference to the temperature of the seasons, led Professor Phillips to the following formula for calculating the difference between the ratios of the quantities of rain received on the ground and, at any height h, the value of the coefficient p depending upon the temperature t' of the season:

$$d = p\,h \frac{t'}{110}$$

Calculated by means of this formula, the mean height of the point at which rain begins to be formed, is 1747 feet; and the height at which the quantity of rain is only one-half of that which falls on the ground, is 356 feet.

Assuming the mean temperature of newly fallen rain at York to be 48°; and taking the latent heat of vapour at 1210° at the temperature of 32° Fahrenheit—the value adopted by Professor Espy in his Meteorological Reports and Essays,—it will be found that a rain-drop cannot acquire the increase of size indicated by the observations, by the condensation of vapour upon its surface, unless its temperature, when at a height above the ground not exceeding that of the top of the tower of York Minster, is below —434° Fahrenheit! From this result it is evident that only a very small portion of the total augmentation of a rain-drop can be due to the condensation of vapour upon its surface, and that by far the greater portion must be owing to the deposition of moisture which has already lost its latent heat, or heat of elasticity, and which is, therefore, not in the sate of a true vapour, although on the other hand, its invisibility in the atmosphere under ordinary circumstances, in the form of cloud or fog, renders it difficult to suppose that it can be in the ordinary liquid state. We have just seen that at a height of 356 feet, the quantity of rain is only one-half of that which falls on the ground; and it is evident, therefore,

that a shallow stratum of the lower and comparatively clear atmosphere, supplies as much rain as a densely clouded, and much deeper stratum in the higher regions. As these remarkable results may raise doubts as to the general correctness of the methods of observation, which have been used to determine the quantities of rain at different heights, I may here mention an important fact, for which I am indebted to my friend Mr. Binney, F.R.S. In descending the shafts of deep coal mines, Mr. Binney has observed that the drops of water which drip from the upper part of the shaft increase to an extraordinary size during their descent to the bottom. Evidently the same principle is here in operation as in the case of a rain-drop falling through the atmosphere, and Mr. Binney's observation affords a valuable confirmation of the general accuracy of the results of the observations which have been made to determine the rain-fall at different elevations.

That the whole amount of a fall of rain is not derived from the direct condensation of vapour at the time that the fall takes place, is apparent from other considerations than those which depend upon the different quantities of rain at different heights. It is supposed by some meteorologists that the mild temperatures of the higher latitudes of western Europe are due to the heat which is liberated by the condensation of vapour during the frequent precipitations of rain which take place on or near the coasts; but if this view were correct, the mean temperature of rainy days ought to be considerably greater than the mean temperature of the year.

A discussion of the Greenwich observations for the years 1852, 3, and 4, and of the Oxford observations for 1855, 6, and 7, with reference to this point, has given the following results:

Greenwich Observations.

Year	Number of Rainy Days	Mean Temp. of Rainy Days	Mean Temp. of the Year
1852	152	51°·39	50°·66
1853	184	47·62	47·49
1854	145	48·80	48·80
General Means.	160·3	49·27	48·98

Oxford Observations.

Year	Number of Rainy Days	Mean Temp. of Rainy Days	Mean Temp. of the Year
1855	140	49°·84	47°·10
1856	154	49·09	48·70
1857	146	49·96	50·40
General Means.	146·6	49·63	48·73

It appears, therefore, that the excess of mean temperature of rainy days, over the mean temperature of the year, on an average of three years, is only 0°·29 by the Greenwich observations, and 0°·90 by the Oxford observations; but as the winds which bring rain come principally from warmer latitudes, the mean temperature of rainy days ought, on that account alone, to be greater than the mean temperature of the year. Dividing the winds into two groups, northerly and southerly, it appears from the Oxford observations that out of 218·5 days of fair weather in the year, the wind was from the northern half of the compass on 131·5 days, and from the southern on the remaining 87; but out of 146·5 rainy days the wind was from the northern half on only 64·5 days, and from the southern on 82. Moreover, the quantity of rain which fell with winds from the southward was nearly four-tenths greater than that which fell with winds from the northward. Calculating the mean temperature of rainy days from the mean temperatures of the winds which prevail on those days, the result is 50°·05; but we have seen that

the observed mean temperature is only 49°·63, or 0°·45 less than the computed. It appears, therefore, that a wind accompanied with rain is, in general, sensibly cooler than the same wind attended with fair weather, and that whatever may be the mode of formation of rain it may be regarded as a cooling process; and this view is borne out by the fact that the mean temperature of the days next after days of rain is sensibly less than that of the days of rain. According to the Greenwich observations the diminution is 0°·29, and according to the Oxford observations it is 0°·19. But if the vapour brought by a rainy wind retains its latent heat up to the moment that actual precipitation of rain takes place, the sudden disengagement of this heat, although occurring in the higher regions of the atmosphere, ought to have a very sensible effect in raising the mean temperature of rainy days; but as no such effect is produced we may conclude that the greater portion, if not the whole, of the moisture from which the rain is formed, had previously lost all its latent and also a small portion of its sensible heat.

The questions now arise — 1st, What becomes of the enormous quantity of heat given off by the vapour which is condensed in the atmosphere? and 2nd, As the moisture which forms rain is not in the state of a true vapour, is it in the ordinary liquid state, or in some other state not hitherto recognised by meteorologists and chemists? With regard to the first question, it may be remarked that air nearly saturated with vapour, has probably a greater power of radiating heat than dry air. The upper portion of a wind charged with vapour would therefore undergo a rapid cooling, and as the vapour which loses its latent heat does not immediately affect the transparency of the air, this process would go on unchecked for some time, and would gradually extend to the lower strata; the vapour which had lost its latent heat would also gradually

descend and accumulate in the lower atmosphere, until at a certain stage of the process clouds and rain were formed. This view of the subject is supported by the well-known fact, that the rate of decrease of the temperature of the atmosphere with the height, is greater in rainy than in fine weather; and it appears likely to lead to a satisfactory explanation of many important atmospherical phenomena.

With respect to the second question, it is difficult to offer any plausible conjecture. There can, however, be little doubt that vapour deprived of its latent heat often exists to a considerable extent in the atmosphere without sensibly affecting its transparency; and, indeed, it often happens that the atmosphere is unusually transparent immediately before, and even during showers of rain, and when, therefore, it is strongly charged with vapour in this peculiar state.

Notwithstanding the cooling by radiation of the upper portion of a warm, moist wind, it is very probable that at a station on the surface of the earth, the temperature would be found to go on slowly increasing, in consequence of the continual arrival of fresh warm air, until the moment when rain began to fall; the rise would then receive a check, and if the rain continued, a decided fall of temperature would take place. If, therefore, we take a day of rain, the day before and the day after, the difference of the mean temperatures of the day of rain and the day before, ought to be less than that of the mean temperatures of the day of rain and the day after. It will be seen that this conclusion is borne out by the following results of the Greenwich and Oxford observations:

	Mean Temp. of Day before Rain	Mean Temp. of Day of Rain	Mean Temp. of Day after Rain
Greenwich observations	49°·25	49°·27	48°·98
Oxford ,,	49·50	49·63	49·44

Should the supposition, that a considerable portion of the aqueous vapour in the atmosphere may lose its latent heat without becoming visible, as cloud or fog, be held to be inadmissible, it appears to me that we shall then have no alternative but to conclude that the generally received theory of latent heat is inapplicable to meteorological phenomena, — a conclusion at least as questionable as the view which I have ventured to advance.

XXIV.—*On the Structure of the Luminous Envelope of the Sun.*

By JAMES NASMYTH, ESQ., C.E.

In a Letter to JOSEPH SIDEBOTHAM, Esq.

Read March 5th, 1861.

THINKING that it might interest some of your scientific friends to be informed about the remarkable details which I have discovered in the general luminous surface of the sun, as also in the structure of the solar spots, I send you for that purpose a rough but faithful drawing, which I hope may serve to convey a pretty clear idea of the details I refer to.

In order to obtain a satisfactory view of these remarkable objects, it is not only requisite to employ a telescope of very considerable power and perfection of defining capability, but also to make the observation at a time when the atmosphere is nearly quite tranquil and free from those vibrations which so frequently interpose most provoking interruptions to the efforts of the observer; without such conditions as I allude to, it is hopeless to catch even a glimpse of these remarkable and delicate details of the solar surface.

The drawing I send you represents a spot on the sun, which I had a most favourable opportunity of observing on the 20th July 1860; although it is in some respects a rather remarkable spot, yet it may be taken as a fair

average type of those of the larger class in their general aspect.

The chief object which I have had in making this drawing is, to exhibit (so far as such a drawing can enable me to do so) those remarkable and peculiar "willow-leaf" shaped filaments of which I find *the entire* luminous surface of the sun to be formed.

The filaments in question are seen and appear well defined at the edges of the luminous surface where it overhangs "the penumbra," as also in the details of the penumbra itself, and most especially are they seen clearly defined in the details of "the bridges" as I term those bright streaks which are so frequently seen stretching across from side to side over the dark part of the spot.

I accompany the drawing with a diagram (No. 2) which exhibits in a more definite and clear manner the exact form of those remarkable structural details of the solar surface.

So far as I have as yet had an opportunity of estimating their actual magnitude, their average length appears to be about 1000 miles, the width about 100.

Diagram No. 2 conveys a pretty clear idea of the manner in which these remarkable details are arranged, in forming, as they do, the entire luminous surface of the sun.

There appears no definite or symmetrical arrangement in the manner in which they are scattered over the surface of the sun; they appear to lie across each other in all possible variety of directions. The thickness of the layer does not appear to be very deep, as I can see down through the interstices which are left here and there between them, and through which the dark or penumbral stratum is rendered visible. It is the occurrence of the infinite number of these interstices, and the consequent visibility of a corresponding portion of the dark or penumbral stratum, that

gives to the general solar surface that peculiar and well known mottled appearance which has for a long time been familiar to the observers of the sun.

You will note that I consider the penumbra to be a true secondary stratum of the sun's luminous envelope, and that what is termed the penumbra of a spot is simply a portion of it, revealed to us by the removal so far of the external and most luminous envelope.

A slight approach to symmetrical arrangement of the details may be observed at the edges of the exterior luminous envelope as it appears surrounding the edge of the spot, and the same may be seen at the edges of the penumbra; the tendency to symmetrical arrangement being a slight approach to a radial formation; the filaments tending in their general position, at the parts in question, to the average centre of the spot.

As I have before said, nothing like a tendency to symmetrical arrangement is observed in the filaments that form the entire luminous surface of the sun. Diagram No. 2 conveys in this respect a very faithful representation.

I may also here note that, although I have most carefully watched for it, I have never seen any indication of a vortical or spiral arrangement of the filaments within or about any of the solar spots; this observation appears to set aside all likelihood of any whirlwind-like action being an agent in the formation of the spots, as has been conjectured was the case.

When a solar spot is mending up, as was the case with the one represented in the drawing, these luminous filaments, or willow-leaf shaped objects (as I term them), are seen to pass from the edges and extend across the spots, thus forming what I term "the bridges" or bright streaks across the spots; if these are carefully observed under favourable conditions, the actual form of these remark-

able details of which "the bridges" are composed will be revealed to sight.

You will also observe that the details of the penumbral portion of the spot are slightly varied in brightness; that portion of the penumbra immediately under the bright edges of the external luminous envelope is less bright than the part of the penumbra next the dark centre of the spot. This is not a mere effect of contrast, but an actual variation in brightness.

You will also notice that portions of the details of the penumbra are in patches considerably brighter than the rest. This effect appears to me to be due to such portions of the penumbra, or the filaments forming it, being more elevated, and consequently brought up into more close contact with the luciferous atmosphere which I am of opinion surrounds the sun, and excites, by some peculiar action, the willow-leaf shaped filaments into full luminosity. This of course is only conjecture at present, but I have some pretty strong grounds for entertaining this view of the subject.

I have also indicated in the drawing a portion of the third luminous envelope, which you will observe like a *mist* underneath some portion of the penumbra. This mysterious object is very difficult to catch a glimpse of, as its comparative brightness is of so very low an order that it is but faintly distinguishable from the darkest portion of the centre of the spot.

I do not as yet feel warranted to hazard any conjecture as to the nature and special functions of those remarkable willow-leaf shaped details of the solar surface which I have discovered, and have attempted to describe to you. I hope however to pursue the investigation of this most interesting subject with all due assiduity this summer, and trust I may be fortunate enough to obtain further insight into their nature. In the meantime, I hope the

hasty description I have endeavoured to give you may prove in some degree interesting, and excite some of our "observers" to devote a little more of their attention to the glorious centre of our system than, I am sorry to say, has been the case hitherto.

Appendix.

MICROSCOPICAL SECTION

OF THE

MANCHESTER LITERARY AND PHILOSOPHICAL

SOCIETY.

ANNUAL REPORT.

1860–61.

The third annual report of your Council presents an opportunity for congratulation upon the steady progress of the Section, especially on the more regular attendance at the meetings, and the more interesting nature of its proceedings. The difficulties attending its establishment appear to be overcome, and a career of usefulness is opening to it, which may prove important to the progress of microscopical investigation.

During the past year two members have been removed by death, Mr. Thompson and Mr. Long. The former had few opportunities of attending the meetings of the Section, but he took great interest in its proceedings. Mr. Long was a member of your Council; he was an ardent follower of scientific pursuits, and his loss is deeply felt by all who knew him. One resignation has been accepted. Three new members have been elected. Several gentlemen have become members of the Parent Society in order to be qualified for joining the Section, and the names of six candidates are now before you for election.

Your Secretary has been elected a member of the Council of the Parent Society, which may be regarded as a compliment to the Section, and a proof of the estimation in which it is held.

During the Session the Section has held two summer and eight ordinary meetings, at which several papers have been read, much valuable information communicated, and

many specimens exhibited. A pleasant excursion was made to Croft's Bank, at the invitation of Mr. Hepworth, whose kind reception, display of objects, and extensive microscopical knowledge, will long be remembered by those who partook of his hospitality.

Papers have been read by

Mr. J. B. Dancer, F.R.A.S., "On cleaning and preparing Diatoms, &c., obtained from soundings."

Mr. W. H. Heys "On the Kaloscope."

And by your Secretary upon "Mr. Dale's process for the separation of tallow from soundings."

Addresses have been given on important subjects by your President, by Mr. Binney, Mr. Sidebotham, and others. Many contributions have been received from gentlemen who take an interest in the prosperity of the Section; amongst whom may be named Captain M. F. Maury and Lieutenant Brooke of the United States Navy, Captain Anderson, Mr. W. K. Parker, Dr. Wallich, Professor Agassiz, Dr. Bacon, Mr. Edwards of New York, Mr. Hepworth, and other distinguished scientific men, whose assistance has been highly valued and duly recorded.

The thanks of the Section are due to Mr. Dancer for the unremitting kindness with which he has provided microscopes and objects for use at the meetings; and the Council wish particularly to record their appreciation of his valuable assistance.

Your Secretary has originated a method of collecting specimens of the sea-bottom obtained by captains of vessels from the soundings they take in ascertaining their position on approaching land; and many shipmasters have been furnished with envelopes in which to preserve those specimens for this Section. The plan promises to be highly successful; upwards of eighty specimens have been received from different parts of the world, such as the

English Channel, Mediterranean and Red Seas, Coasts of Portugal and Brazil, Deep Atlantic and Deep North Pacific, Coasts of Japan, &c. &c. Amongst those from the Pacific Ocean are the deepest soundings from which material has yet been brought up from the sea-bottom, say 3,030 fathoms, or nearly $3\frac{1}{2}$ miles; the quantity of material is necessarily small; and so far as yet examined, in this specimen no trace of organic bodies has been found. Arrangements are in progress for the scientific examination and mounting of these soundings, some of which will be laid before you this evening. About 1,200 envelopes have been distributed, mostly amongst captains now out on distant voyages, to the East and West Indies, Coasts of Africa and Australia, as well as to some of the Pacific and Sperm whalers and traders; a few of which may in time be returned with interesting material. It is encouraging to know that other societies are following this example, so that our knowledge of the sea-bottom will soon be vastly increased.

Results of unexpected magnitude are likely to follow these humble efforts to obtain specimens from the deep sea. Amongst those captains who were solicited to preserve their soundings was Captain JAMES ANDERSON, then of the Cunard steamer "Canada." In the course of correspondence with your Secretary, this enlightened sailor developed a long-thought-of plan for the social advancement of his fellow-mariners, to induce them to study natural science in its various branches, and to render their assistance available to scientific institutions throughout the country. Captain ANDERSON asked for your assistance to carry out his views. All who heard his letter, were so convinced of the importance of the project that it was unanimously determined, as a first step, the letter should be printed and circulated at the expense of the Section. That has been done to a limited extent, and in conse-

quence a meeting of a few friends was held in the Liverpool Town Hall on the 30th ultimo. The Mayor, R. S. Graves, Esq., presided. There were present Colonel Wm. Brown, Dr. Collingwood, Captain Anderson, Mr. Rathbone, Mr. Mackay, and other eminent shipowners and gentlemen favourable to the scheme. After Captain Anderson had explained his views, your Secretary endeavoured to point out how societies in interior towns could contribute to its success, and participate in its advantages; how shipmasters would improve themselves by the collection of specimens, and the study of natural sciences in general, but more particularly that of meteorology, to enable them to shorten voyages, and to reduce the losses shipowners and underwriters now constantly suffer. All were deeply impressed with the advantages to be derived if a good working plan could be organised. None could at once be formed without some objections; but a committee was appointed to take the subject into consideration, and report thereon.

It will be a source of gratification to this Section if, through its instrumentality, the first steps were taken to commence a work the importance of which, if thoroughly carried out, will be considerable. To promote scientific research amongst a numerous class of men and youths whose opportunities of collecting specimens and making scientific observations in all parts of the world are unequalled, is an object worthy of our attention; and although another generation may be required fully to develop its usefulness, some good may be done even in our day.

With such purposes in view, the future prospects of our Section are encouraging; and although in the highly scientific branches of microscopical research we have done but little, it is to be hoped that our professional members may, from their stores of experience and study, contribute more liberally to the general fund.

Before the next Session is far advanced the members of the British Association and many distinguished foreigners will be amongst us, and it behoves one and all of our members to make every exertion, that this Section may worthily represent the microscopy of the day, and the city to which we belong.

Manchester,
30*th May*, 1861.

9, *St. Peter's Square*,
Manchester, 4*th April*, 1861.

Sir,

The Microscopical Section of the Manchester Literary and Philosophical Society have by circular requested many Captains of merchant vessels to preserve for microscopical examination, the material brought from the sea-bottom, by the soundings they make in various parts of the world. Amongst others, application was made to Captain Anderson of the Royal Mail Steam Ship "Canada," and he not only entered fully into the spirit of the request, but in course of correspondence with the Secretary, Captain Anderson gave the outline of a project which has occupied his attention for some years past, for cultivating a taste for Natural and Meteorological Science amongst Mariners, and for rendering their assistance available to scientific institutions; he embodied his views in a letter that was read at the meeting of 18th March.

So strong was the feeling of the members upon the subject, it was unanimously resolved that Captain Anderson's letter should be printed and circulated at the expense of the Section, with a view to elicit opinions upon the feasibility of the project, and upon the best practical method of carrying it into execution. In the first instance the concurrence of ship owners and merchants will be required; I shall therefore be glad to be informed if after perusal of the letter, you are disposed to assist the movement. Should sufficient encouragement be obtained it is proposed to call a meeting in Liverpool to organise the plan of further proceedings.

I am, Sir,
Yours truly,
GEORGE MOSLEY,
HON. SEC.

Royal Mail Steam Ship "Canada," at Sea.

February 23rd, 1861.

Dear Sir,

*   *   *

I forwarded the packages of sounding envelopes to Commander Maury and to Mr. Osborn of New York; but allow me to express a doubt if much can be hoped from any such method as applying to whalers or others going upon casual voyages, whose commanders have no inducement to any such industry. I would not express myself thus had I no other plan to offer; but it will involve filling another sheet, and as it is a hobby with me, let me make an appeal to your Society for a fair consideration.

You have on shore Literary and Philosophical Societies, Museums, Free Libraries, Working Men's Associations, Lectures, all kinds of instruction and diversion — for that too is necessary to relieve the over-taxed brain or irritated temper — besides the influence of social intercourse to smooth rough manners, and make man's life run pleasantly along.

All classes and all ages so generally avail themselves of one or other of these privileges, it is fair to infer that variety of occupation is essential to the healthy tone of an active mind — for any of these sailors have no equivalent, but *might* have.

I am not now writing of such a life as mine, ever varying, or of the few who by strong scientific turn of mind, happily directed, have succeeded in possessing themselves of a hobby ever present as a source of recreation and happiness; but I am pleading for many fine minds in the mercantile marine of England, now lost to science for want of hobbies.

To give them intellectual hobbies would be to raise the whole class in the social scale, and I know of no obstacle to that, but the one fear that possesses all of them, that they might be told to let science alone and mind their own

business. Sailors, by their education at sea, are naturally submissive to any one having authority, and it remains with ship-owners to give them that intellectual recreation and direction, if they please. The merchants of Manchester are themselves ship-owners, or have influence amongst them. I am in hopes I am writing what will be read by some such, and if there be some philanthropist who will follow it up, he would find a harvest such as no other scheme can offer to science.

There is a "Mercantile Marine Association" in Liverpool well attended by ship-masters, so that they can be reached as a body at once. I would propose that body issuing diplomas or certificates, such as other Literary Associations use, and giving them to all members who by any industrious application to science or gathering of contributions to scientific associations: — or who by keeping a school on board their ships and bringing forward two or more apprentices to pass an examination, as a proof of their assiduity: — or by qualifying themselves in any language other than their own: — or by a knowledge of Naval Architecture: — or by any kind of industrious application, tend to elevate the mercantile marine of England in the social scale.

It is obvious, however, that these diplomas would be of no value unless countenanced by ship-owners. If they would but say to their commanders, "We would give a preference (other circumstances being equal) to men holding such certificates," I give it as my strong conviction, that it would be largely responded to, and that they would make better men of their ship-masters. A man with a hobby is always safer both at sea and abroad, than a thoroughly idle man.

From the very nature of the duties required of a seaman, by which he may hope to arrive at the position of commander, that station is generally reached with no other

resources than just what constitute a sailor, and then he finds himself an idle man or comparatively so, after his life of unceasing industry. There are many times when ships are becalmed in the Eastern Seas and Pacific, when soundings might be taken, if any one who had a taste for that pursuit were supplied with the necessary apparatus, partly at the expense of the Association to which he belonged, and in part by the Society desiring such soundings, we might soon know the nature of the deep sea-bottom. Indeed, for collecting specimens of every thing in the sea and out of it, from the first germ to the finished animal or fabric, whether in Comparative Anatomy, Zoology, Ethnology, Botany or Meteorology, no better adapted staff of workers could well be wished for: they carry your fabrics to every corner of the world; and to my thinking there is but one thing necessary—let the owners desire their commanders to pay some attention in their leisure time to some of these pursuits, and approve of their holding the certificate of the Association.

It is too much to expect that they will apply themselves, and neither find their pride gratified or their industry acknowledged; but with this or some such scheme there is a great deal of good to be done to the class of men to which I belong. As a matter of social science I hope some of your members may take it up.

I hope you do not think you have found one likely to bore you frequently with such long yarns. I am in earnest on this point, having thought of it for years; and were I not trying to be brief I could go on and shew how I would supply the Museums of Liverpool and Manchester so as to exceed all others in their completeness; but I should like what I have written to be read, and if it is not afterwards thought of, then it is long enough.

Yours very truly,

JAMES ANDERSON.

SESSIONS 1859–60 AND 1860–61.

LIST OF DONATIONS RECEIVED

From 1859 April 19th, to 1861 April 29.

From Scientific Societies and Institutions in Great Britain and Ireland, with Colonies.

BATH. *The Bath and West of England Society of Agriculture, &c.:* Journal of the Society, New Series, Vol. VIII. Parts I. and II. London 1860. 8vo

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		Appendix to the Makerstoun Magnetical and Meteorological Observations. (Supp. to Vol. XXII. of the Transactions.) Edinburgh 1860. 4to
		Proceedings of the Society, Vol. IV. Nos. 49 and 50. 8vo
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1843. Apr. 18.	Barlow, Peter, F.R.S., F.R.A.S., Hon. M.C.P.S., Mem. Imper. Acad. Sc. Petersburg, and Amer. Acad. Arts and Sc. Boston, Corr. Mem. Imper. Instit. France and Roy. Acad. Sc. Brussels. *Woolwich, London, S.E.*
1849, Jan. 23.	Bosworth, Rev. Joseph, LL.D., F.R.S., F.S.A., M.R.I.A., Corr. Mem. Roy. Soc. Northern Antiq. Copenhagen, Mem. Roy. Soc. Sc. Drontheim and Soc. Sc. Gothenburg. *Islip House, near Oxford, and Etwell, Uttoxeter.*
1843, Apr. 18.	Brewster, Sir David, K.H., LL.D., F.R.SS. L. & E., Hon. M.R.I.A., F.G.S., F.R.A.S., Chev. of the Prussian Order "*Pour le Mérite,*" Mem. Imper. Instit. France and Roy. Soc. Sc. Gottingen, Principal of St. Leonard's College. *St. Andrews.*

DATE OF ELECTION.

1860, Apr. 17. Bunsen, Robert Wilhelm, Ph.D., Prof. of Chemistry at the Univ. of Heidelberg. *Heidelberg.*

1859, Jan. 25. Cayley, Arthur, M.A., F.R.S., F.R.A.S. 2, *Stone Buildings, Lincoln's Inn, London, W.C.*

1859, Jan. 25. De Morgan, Augustus, V.P.R.A.S., F.C.P.S., Professor of Mathematics in Univ. Coll. London. 41, *Chalcot Villa, Adelaide Road, London.*

1844, Apr. 30. Dumas, Jean Baptiste, F.R.S., Mem. Imper. Instit. France, &c. *Paris.*

1843, Apr. 18. Faraday Michael, D.C.L., F.R.S., Fullerian Prof. of Chemistry in the Roy. Instit. of Great Britain, Hon. Mem. R.S.E., C.P.S., and Med. Chir. Soc., F.G.S., Chev. of the Prussian Order "*Pour le Mérite*," Comm. Legion of Honour, Mem. Imper. Instit. France, Imper. Acadd. Sc. Vienna and Petersburg, Roy. Acadd. Sc. Berlin, Turin, Stockholm, Munich, Naples, Amsterdam, Brussels, Bologna and Modena, Roy. Socc. Sc. Gottingen, Copenhagen, Upsal and Haerlem. Amer. Acad. Arts and Sc. Boston, and Amer. Phil. Soc. Philadelphia, Corr. Mem. Acad. Sc. and Liter. Palermo, Acad. Georg. Florence, Philom. Soc. Paris, Nat. Instit. Washington, U.S., and Imper. Acad. of Med. Paris, &c. 21, *Albemarle Street, London, W.*

1860, Jan. 24. Fries, Elias, A.M., Prof. Emer. at the Univ. of Upsala, Comm. of the Swedish Order of the "*Northern Star*," Chev. of the Danish Order of "*Dannebrog*," Mem. Swed. Acad., Roy. Acad. Sc. Stockholm and Roy. Soc. Sc. Upsala, &c. &c. *Upsala.*

1843, Feb. 7. Frisiani, nobile Paolo, Prof., 2nd Astron. at the Observ. of Brera, Milan, Mem. Imper. Roy.

DATE OF ELECTION.

Instit. of Lombardy, Milan, and Ital. Soc. Sc. *Milan.*

1861, Jan. 22: Haidinger Wilhelm Karl, Ph.D., M.D., Aulic Councellor, Director of the I. R. Geol. Inst. Vienna, Chev. of the Austrian Order "*Franz Joseph*," and several other Orders, F.R.SS. L. & E., Corr. Mem. Imp. Instit. France, Mem. Imp. Acad. Sc. Vienna, Hon. Mem. I. R. Soc. Phys. Vienna, Roy. Geogr. Soc. London, Imp. Geogr. Soc. St. Petersburg, Roy. Soc. Nat. Sc. and Geol. Soc. of Hungary, &c. &c., Mem. Roy. Acad. Sc. Stockholm, Munich and Brussels, Roy. Soc. Sc. Gottingen and Copenhagen, &c. &c., Corr. Mem. Imper. Acad. Sc. St. Petersburg, I. R. Inst. Sc. Venice, Roy. Acadd. Sc. Berlin and Turin, Roy. Inst. of Lombardy, Milan, Roy. Caled. Hortic. Soc. Edinburgh, Imper. Soc. Nat. Sc. Cherbourg, Soc. Sc. Batavia, Acad. Nat. Sc. Philadelphia. 363, *Landstrasse, Ungergasse, Vienna.*

1843, Feb. 7. Hamilton, Sir William Rowan, Knt., LL.D., M.R.I A., F.R.A.S., Astronomer Royal, Ireland, Andrews Professor of Astronomy, T.C.D. *Observatory, Dunsink, Dublin.*

1843, Feb. 7. Harcourt, Rev. William Venables Vernon, M.A., F.R.S., Hon. M.R.I.A., F.G.S. *Bolton Percy, Tadcaster.*

1853, Apr. 19. Hartnup, John, F.R.A.S. *Observatory, Liverpool.*

1843, Apr. 18. Herschel, Sir John Frederick William, Bart., M.A., D.C.L., F.R.SS. L. & E., Hon. M.R.I.A., F.G.S., M.C.P.S., Chev. of the Prussian Order "*Pour le Mérite*," Corr. Mem. Imper. Instit. France, Mem. Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Berlin, Turin, Naples and Brussels, Roy. Socc. Sc. Gottingen, Copenhagen and Haerlem, Acad. Dei. Nuovi Lincei

DATE OF ELECTION.

Rome, Acadd. Padua, Bologna, Palermo, Modena, Acad. Gioen. Catania, Imper. Acad. Sc. Dijon, Philom. Soc. Paris and Soc. Nat. Sc. Switzerland. *Collingwood, near Hawkhurst, Kent.*

1848, Jan. 25. Hind, John Russell, F.R.A.S., Superintendent of the Nautical Almanac. 22, *Grove-road, St. John's Wood, London.*

1853, Jan. 25. Hopkins, William, M.A., LL.D., F.R.S., F.G.S. *Fitzwilliam Street, Cambridge.*

1852, Oct. 19. Kirkman, Rev. Thomas Penynton, M.A., F.R.S. *Croft Rectory, near Warrington.*

1848, Oct. 31. Lassell, William, F.R.S., F.R.A.S., Hon. F.R.S.E., Hon. M. Philomath Soc. Paris. *Bradstones, Sandfield Park, near Liverpool.*

1847, Apr. 20. Le Verrier, Urbain Jean Joseph, F.R.S., Mem. Imper. Instit. France, &c. *Paris.*

1843, Feb. 7. Liebig, Justus Baron Von, M.D., Ph.D., Prof. of Chem. Univ. Munich, Conservator of Chem. Labor. Munich, Chev. of the Bav. Order "*Pour le Mérite*," &c., F.R.SS. L. & E., Hon. M.R.I.A., Hon. Mem. Univ. Dorpat and Med. Phis. Facult. Univ. Prague, Hon. Mem. and For. Assoc. Imper. Acad. Sc. Vienna, Roy. Acadd. Sc. Stockholm, Brussels, Amsterdam, Turin, Acad. Sc. Bologna, Roy. Socc. Sc. Gothenburg, Gottingen, Copenhagen, Liège, Imper. Roy. Instit. of Lombardy, Milan, Corr. Mem. Imper. Instit. France, Imper. Acad. Sc. Petersburg, Roy. Acad. Sc. Madrid, Mem. Roy. Med. Chir. Soc. London and Perth, Roy. Scot. Soc. Arts, Botan. Socc. Edinburgh and Regensburg, Socc. Nat. Sc. Berlin, Dresden, Halle, Moscow, Lille, Ph. Soc. Glasgow, Agric. Socc. Munich, Giessen, &c. &c. *Munich.*

DATE OF ELECTION.

1849, Apr. 17. Mercer, John, F.R.S. *Oakenshaw, Accrington, Lancashire.*

1843, Feb. 7. Mitscherlich, Eilert, Professor, F.R.S., &c. *Berlin.*

1854, Jan. 24. Morin, Arthur, General of Artillery, Mem. Imper. Instit. France, formerly élève Polytechn. School, Dir. Conserv. of Arts, Paris, Corr. Mem. Roy. Acadd. Sc. Berlin, Madrid and Turin, Acad. Georg. Florence, Imper. Acad. Mentz, and Industr. Soc. Mulhouse. *Paris.*

1843, Apr. 18. Moseley, Rev. Henry, M.A., F.R.S., Corr. Mem. Imper. Instit. France. *Olveston, near Bristol.*

1821, Jan. 26. Mosley, Sir Oswald, Bart. *Rolleston Hall.*

1844, Apr. 30. Murchison, Sir Roderick Impey, G.C.St.S., D.C.L., M.A., F.R.S., F.G.S., F.L.S., &c., Director Gen. of the Geol. Survey, Pr. R.G.S., Hon. Mem. R.S.E., and R.I.A., Mem. C.P.S. and Imper. Acad. Sc. Petersburg, Corr. Mem. Imper. Instit. France, Roy. Acadd. Sc. Stockholm, Turin, Berlin and Brussels, Roy. Soc. Sc. Copenhagen, Amer. Acad, Arts and Sc. Boston, and Imper. Geogr. Soc. Petersburg, Hon. Mem. Imper. Soc. of Naturalists, Moscow. &c. &c. 16, *Belgrave Square, London, S.W.*

1844, Apr. 30. Owen, Richard, M.D., LL.D., V.P.R.S., F.L.S., F.G.S., V.P.Z.S., Director of the Nat. Hist. Department, British Museum, Hon. F.R.C.S. Ireland, Hon. M.R.S.E., For. Assoc. Imper. Instit. France, Mem. Imper. Acadd. Sc. Vienna and Petersburg, Imper. Soc. of Naturalists Moscow, Royal Acadd. Sc. Berlin, Turin, Madrid, Stockholm, Munich, Amsterdam, Naples, Brussels and Bologna, Roy. Socc. Sc. Copenhagen and Upsal, and Amer. Acad. Arts and Sc. Boston, Corr. Mem. Philom. Soc. Paris, Mem. Acad. Georg. Florence, Soc. Sc. Haerlem

DATE OF ELECTION.

and Utrecht, Soc. of Phys. and Nat. Hist. Geneva, Acad. dei Nuovi Lincei Rome, Roy. Acadd. Sc. Padua, Palermo, Acad. Gioen. Catania, Phys. Soc. Berlin, Chev. of the Prussian Order "*Pour le Mérite*," For. Assoc. Instit. Wetter., Philadelphia, New York, Boston, Impér. Acad. Med. Paris, and Imper. and Roy. Med. Soc. Vienna. *British Museum, London, W.C.*

1851, Apr. 29. Playfair, Lyon, C.B., Ph.D., F.R.S., F.G.S., F.C.S., Professor of Chemistry Univ. Ed. *Edinburgh.*

1856, Jan. 22. Poncelet, General Jean Victor, F.R.S., Mem. Imper. Instit. France, &c. *Paris.*

1859, Apr. 19. Rankine, William John Macquorn, LL.D., F.R.SS. L. & E., Pres. Inst. Eng. Scot., Regius Professor of Civil Engineering and Mechanics, Univ. Glasgow. *59, St. Vincent Street, Glasgow.*

1849, Jan. 23. Rawson, Robert. *Royal Dockyard, Portsmouth.*

1859, Apr. 19. Reichenbach, Carl Baron von. *Vienna.*

1861, Apr. 30. Roberts, Richard, M. Inst. C.E. *Adam Street, Adelphi, London.*

1844, Apr. 30. Sabine, Major-General Edward, R.A., D.C.L., Treas. and P.R.S., F.R.A.S., Hon. Mem. C.P.S., Chev. of the Prussian Order "*Pour le Mérite*," Mem. Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Berlin, Brussels and Gottingen, Roy. Soc. Sc. Drontheim, Acad. Sc. Philadelphia, Econ. Soc. Silesia, Nat. Hist. Soc. Lausanne and Roy. Batavian Soc., Corr. Mem. Roy. Acad. Sc. Turin, Nat. Instit. Washington, U.S., Geogr. Socc. Paris, Berlin and Petersburg. 13, *Ashley Place, Westminster, London, S.W.*

1843, Feb. 7. Sedgwick, Rev. Adam, M.A., F.R.S., Hon.

DATE OF ELECTION.

M.R.I.A., F.G.S., F.R.A.S., Woodwardian Lecturer. *Cambridge.*

1851, Apr. 29. Stokes, George Gabriel, M.A., D.C.L., Secr. R.S., Lucasian Professor of Mathem. F.C.P.S., Mem. Batav. Soc. Rotterdam. *Pembroke College, Cambridge.*

1861, Jan. 22. Sylvester, James Joseph, Esq., M.A., F.R.S., Professor of Mathematics at the Royal Military Acad. *Woolwich.*

1854, Jan. 24. Tayler, Rev. John James, B.A., Principal of Manchester New College. *London.*

1851, Apr. 29. Thomson, William, M.A., LL.D., F.R.SS. L. & E., Professor of Nat. Philos. Univ. Glasgow. 2, *College, Glasgow.*

1843, Feb. 7. Whewell, Rev. William, D.D., F.R.S., Hon. M.R.I.A., F.S.A., F.G.S., F.R.A.S., Master of Trinity College, Cambridge. *Lodge, Cambridge.*

1850, Apr. 30. Woodcroft, Bennet, Professor, Superint. of Regist. of Patents. *Southampton Buildings, London, W.C.*

CORRESPONDING MEMBERS.

1860, Apr. 17. Ainsworth, Thomas, *Cleator Mills, Near Egremont, Cumberland.*

1861, Jan. 22. Buckland, George, Professor, University College, *Toronto.*

1824, Jan. 23. Dockray, Benjamin, *Lancaster.*

1861, Apr. 2. Fardel, Max. Durand, M.D., Chev. of the "*Legion of Honour*," &c.

1849, Apr. 17. Girardin, M. J., Corr. Mem. Impér. Instit. France, *Rouen.*

DATE OF ELECTION.

1850, Apr. 30. Harley, Rev. Robert, F.R.A.S., *Brighouse, Yorkshire.*

1861, Jan. 22. Henry Joseph, Professor, Sec. Smithsonian Institute, *Washington, U.S.*

1816, Apr. 26. Kenrick, Rev. John, M.A., *York.*

1838, Apr. 17. Koechlin-Schouch, Daniel, *Mulhouse.*

1859, Jan. 25. Le Jolis, Augustus, Hon. Perpet. Archiviste Impér. Soc. Nat Sc. Cherbourg, Mem. Impér. Léopold-Carol. Acad. Nat. Sc., *Cherbourg.*

1857, Jan. 27. Lowe, Edward Joseph, F.R.A.S., F.G.S., M.B.M.S., Hon. M. Dublin Nat. Hist. Soc., M. Geol. Soc. Edinb., &c., *Nottingham.*

1851, Apr. 29. Pincoffs, Peter, M.D., Chev. of the Turkish Order of the "*Medjidié*," 4th Cl., Mem. Coll. Phys. London, Brussels and Dresden, Hon. and Corr. Mem. Med. and Phil. Socc. Antwerp, Athens, Brussels, Constantinople, Dresden, Rotterdam, Vienna, &c. &c., *Constantinople.*

1808, Nov. 18. Roget, Peter Mark, M.D., F.R.S., F.G.S., F.R.A.S., V.P.S.A., Mem. Roy. Coll. Phys., Corr. Mem. Roy. Acad. Sc. Turin, 18, *Upper Bedford Place, London, W.C.*

OMISSIONS IN LIST OF CORRESPONDING MEMBERS.

DATE OF ELECTION.

1834, Jan. 24. Watson, Henry Hough. *Bolton, Lancashire.*

1851, Apr. 19. Wilkinson, Thomas Turner, F.R.A.S. *Burnley, Lancashire.*

ALPHABETICAL LIST OF THE MEMBERS OF THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

April 30th, 1861.

DATE OF ELECTION.	
1857, Jan. 27.	Acton, Henry Morell, B.A. *Office of the Manchester Guardian.*
1839, Apr. 30.	Ainsworth, Ralph Fawcett, M.D. *Union Club, Mosley-street.*
1861, Jan. 22.	Alcock, Thomas, M.D. 66, *Upper Brook-street, Chorlton-on-Medlock.*
1854, Jan. 24.	Allan, James, Ph.D., M.A. 33, *Victoria-street, Sheffield.*
1861, Jan. 22.	Anson, Rev. G. H. Greville, M.A. *Rector of Birch, near Rusholme.*
1837, Aug. 11.	Ashton, Thomas. 42, *Portland-street, Manchester.*
1846, Jan. 27.	Atkinson, John. *Thelwall, near Warrington.*
1824, Jan. 23.	Barbour, Robert. 18, *Aytoun-street, Portland-street.*
1852, Jan. 27.	Barlow, Henry Bernoulli. 11, *Ducie-street, Market-street, Manchester.*
1842, Apr. 19.	Barratt, Joseph. *Birkdale, Southport.*
1849, Apr. 17.	Bassnett, Rev. Richard, A.M. *Gorton, near Manchester.*
1840, Jan. 21.	Bateman, John F., F.R.S., M. Inst. C.E., F.G.S. 16, *Great-George-street, Westminster, S. W.*
1858, Jan. 26.	Baxendell, Joseph, F.R.A.S. 108, *Stocks-street.*
1847, Jan. 26.	Bazley, Thomas, M.P. *Water-street Mills, Water-street, Manchester.*
1847, Jan. 26.	Bell, William. 51, *King-street, Manchester.*

DATE OF ELECTION.

1857, Apr. 21. Bellhouse, Edward Taylor. *Hunt-street, Brook-street, near Oxford-road.*

1858, Jan. 26. Benson, Davis. *Sugar Works, Chester-street, Oxford-road.*

1844, Jan. 23. Bevan, James. *Hesketh-street, Southport.*

1854, Jan. 24. Beyer, Charles. 9, *Hyde-road, Ardwick.*

1842, Jan. 25. Binney, Edward William, F.R.S., F.G.S. 40, *Cross-street, Manchester.*

1821, Jan. 26. Blackwall, John, F.L.S. *Hendre, Llanrwst, Wales.*

1859, Jan. 25. Brittain, Thomas. 3, *Peel-street, Cannon-street.*

1861, Jan. 22. Bottomley, James. 2, *Nelson-street, Lower Broughton.*

1855, Jan. 23. Bowman, Eddowes, M.A. *Victoria Park, Rusholme.*

1839, Oct. 29. Bowman, Henry. *Victoria Park, Rusholme.*

1855, Apr. 17. Brockbank, William. 37, *Princess-street, Manchester.*

1861, Apr. 2. Brogden, Henry. *Brooklands, near Sale.*

1844, Jan. 23. Brooks, William Cunliffe, M.A. *Bank*, 92, *King-street.*

1860, Jan. 24. Brothers, Alfred. 14, *St. Ann's Square, Manchester.*

1846, Jan. 27. Browne, Henry, M.D. 21, *Lever-street, and Oxford-road.*

1861, Jan. 22. Buckley, Rev. Thomas, M.A. *Old Trafford.*

1854, Jan. 24. Callender, William Romaine, Jun. F.S.A. 2, *Charlotte-street, Manchester.*

1847, Jan. 26. Calvert, Frederick Crace, Ph.D., F.R.S., F.C.S., Corr. Mem. Roy. Acad. Sc. Turin, Acad. Sc. Rouen, Pharmac. Soc. Paris and Industr. Soc. Mulhouse. *Royal Institution, Bond-street, Manchester.*

1859, Jan. 25. Carrick, Thomas. 37, *Princess-street, Manchester.*

1858, Jan. 26. Casartelli, Joseph. 43, *Market-street, Manchester.*

1855, Jan. 23. Cawley, Charles Edward, M. Inst. C.E. 41, *John Dalton-street, Manchester.*

1852, Apr. 20. Chadwick, David, F.S.S., Assoc. Inst. C.E. 75, *King-street, Manchester.*

DATE OF ELECTION.	
1842, Jan. 25.	Charlewood, Henry. *Clarence Chambers, Clarence-street.*
1857, Apr. 21.	Churchill, George Cheetham. 86, *Cross-street, Manchester.*
1854, Apr. 18.	Christie, Richard Copley, M.A., Prof. Hist. Owens College. 7, *St. James's-square, Manchester.*
1841, Apr. 20.	Clay, Charles, M.D. 101, *Piccadilly, Manchester.*
1861, Jan. 22.	Clifton, Robert Bellamy, B.A., Prof. Nat. Phil. Owens College. *Owens College, Manchester.*
1853, Apr. 19.	Clift, Samuel, F.C.S. *Bethel's Chemical Works, Ashton-road.*
1853, Jan. 25.	Corbett, Edward. *Cross-street Chambers*, 78, *Cross-street, Manchester.*
1853, Jan. 25.	Cottam, Samuel. 28, *Brazenose-street, Manchester.*
1859, Jan. 25.	Coward, Edward. *Heaton Mersey, near Manchester.*
1851, Apr. 29.	Crompton, Samuel. *Cavendish-street, Stretford-road.*
1839, Jan. 22.	Crossley, James, F.S.A., Pres. Chet. Soc. 6, *Booth-street, Piccadilly, Manchester.*
1848, Jan. 25.	Crowther, Joseph S. *Messrs. Bowman and Crowther*, 22, *Princess-street.*
1861, Apr. 2.	Cunningham, William Alexander. *Manchester and Salford Bank, Mosley-street.*
1843, Apr. 18.	Curtis, Matthew. *Phœnix Works, Chapel-street, Ancoats.*
1861, Jan. 22.	Curtis, John. 24A, *York-street, Manchester.*
1854, Feb. 7.	Dale, John. *Messrs. Roberts, Dale & Co., Cornbrook.*
1842, Apr. 19.	Dancer, John Benjamin, F.R.A.S. 43, *Cross-street, Manchester.*
1853, Apr. 19.	Darbishire, Robert Dukinfield. 21, *Brown-street, Manchester.*
1854, Jan. 24.	Davies, David Reynold. *Messrs. George Fraser and Son*, 33, *Dickinson-street.*
1842, Nov. 15.	Dean, James Joseph. 2, *Grove-street, Ardwick.*
1855, Jan. 23.	Dickinson, William L. 1, *St. James's-street, Charlotte-street.*

DATE OF ELECTION.

1859, Jan. 25. Dorrington, James. *Messrs. Fraser and Son*, 33, *Dickinson-street.*

1850, Apr. 28. Dyer, Frederick Nathaniel. 9, *Wellington Place, Longsight.*

1818, Apr. 24. Dyer, Joseph Chesborough. *Burnage, near Levenshulme.*

1859, Jan. 25. Eadson, Richard. *Rochdale Canal Warehouse, Dale-street, Piccadilly.*

1856, Apr. 29. Ekman, Charles Frederick. 41, *George-street, Manchester.*

1854, Jan. 24. Ellis, Charles. 21, *Rook-street, Meal-street, Mosley-street.*

1850, Apr 30. Fairbairn, Thomas. 16, *Booth-street, Mosley-street.*

1824, Oct. 29. Fairbairn, William, C.E., LL.D., F.R.S., Corr. Mem. Imp. Inst. France and Roy. Acad. Sc. Turin. *Canal-street, Ancoats.*

1849, Oct. 30. Fairbairn, William Andrew. *Canal-street, Ancoats.*

1861, Jan. 22. Fisher, William Henry. 16, *Tib Lane, Cross-street.*

1842, Jan. 25. Fleming, David Gibson. *Messrs. R. Barbour and Brothers*, 18, *Aytoun-street.*

1817, Oct. 31. Flint, Richard. *Stockport.*

1856, Apr. 29. Forrest, H. R. 70, *George-street, Manchester.*

1857, Apr. 21. Foster, Thomas Barham. 23, *John Dalton-street.*

1855, Jan. 23. Fothergill, Benjamin. 65, *Cannon-street, London, E.C.*

1860, Apr. 17. Francis, John. *Town Hall, King-street.*

1854, Jan. 24. Fryer, Alfred. *Sugar Works, Chester-street, Oxford-road.*

1840, Jan. 21. Gaskell, Rev. William, M.A. 46, *Plymouth Grove.*

1861, Apr. 30. Gladstone, Murray, F.R.A.S. *Messrs. Gladstone, Latham and Co.*, 24, *Cross-street.*

1860, Apr. 17. Glover, George. *Sun Chambers*, 15, *Market-street.*

DATE OF ELECTION.

1847, Apr. 20. Gould, John. 24, *Legh-place, Ardwick.*

1817, Jan. 24. Greg, Robert Hyde, F.G.S. *Chancery-lane, Booth-street, Mosley-street.*

1849, Oct. 30. Greg, Robert Philips, F.G.S. *Chancery-lane, Booth-street, Mosley-street.*

1848, Jan. 25. Grundy, John Clowes. 4, *Exchange-street, Manchester.*

1844, Jan. 23. Hampson, Richard. *Withington.*

1858, Oct. 19. Harrison, William Philip, M.D. 145, *Great Ducie-street, Strangeways.*

1839, Jan. 22. Hawkshaw, John, F.R.S., F.G.S., M. Inst. C.E. 33, *Great George-street, Westminster, London, S.W.*

1861, Apr. 2. Haywood, George Robert. *Newall's Buildings*, 14, *Market-street, Manchester.*

1859, Apr. 19. Heelis, Thomas, F.R.A.S. 75, *Princess-street, Manchester.*

1828. Oct. 31. Henry, William Charles, M.D., F.R.S. 11, *East-street, Lower Mosley-street, Manchester.*

1854, Jan. 24. Hetherington, John. *Pollard-street, Ancoats.*

1861, Apr. 30. Heys, William Henry. *Hazel Grove, near Stockport.*

1815, Jan. 27. Heywood, Sir Benjamin, Bart., F.R.S. *Claremont, near Manchester.*

1833, Apr. 26. Heywood, James, F.R.S., F.G.S., F.S.A. 26, *Kensington Palace Gardens, London, W.*

1851, Apr. 29. Higgin, James. *Chemical Works, Hulme Hall.*

1845, Apr. 29. Higgins, James. *King-street, Salford.*

1848, Oct. 31. Higson, Peter. 94, *Cross-street, Manchester.*

1839, Jan. 22. Hobson, John. *Bakewell, Derbyshire.*

1861, Apr. 2. Hobson, John Thomas, Ph.D. *Messrs. Thonas Hoyle and Sons, Mayfield Works.*

1820, Jan. 21. Hodgkinson, Eaton, F.R.S., F.G.S., M.R.I.A., Hon. Mem. R.I.B.A. Inst. C.E., Roy. Scot. Soc. Arts, and Soc. Civ. Eng. Paris, Prof. of the Mech. Princ. of Engineering Univ. Coll. London. *Higher Broughton.*

DATE OF ELECTION.

1854, Jan. 24. Holcroft, George. 5, *Red Lion-street, St. Ann's-square.*

1855, Jan. 23. Holden, Isaac. 64, *Cross-street, Manchester.*

1846, Jan. 27. Holden, James Platt. *St. James's Chambers*, 3, *South King-street, Manchester.*

1823, Apr. 18. Hopkins, Thomas, M. Brit. Met. Soc. *Broughton-lane.*

1824, Jan. 23. Houldsworth, Henry. *Newton-street, Ancoats-street, Manchester.*

1857, Jan. 27. Hunt, Edward, B.A., F.C.S. 20, *Devonshire-street, All Saints, Manchester.*

1859, Jan. 25. Hurst, Henry Alexander. *Messrs. Mosley, Hurst and Co.*, 9, *St. Peter's-square.*

1823, Jan. 24. Jesse, John, F.R.S., F.R.A.S., F.L.S. *Lanbedr Hall, near Ruthin, Denbighshire.*

1850, Apr. 30. Johnson, Richard, F.C.S. *Oak Bank, Fallowfield, near Manchester.*

1821, Oct. 19. Jordan, Joseph. 70, *Bridge-street, Manchester.*

1848, Apr. 18. Joule, Benjamin St. John Baptist. *Thorncliff, Old Trafford.*

1842, Jan. 25. Joule, James Prescott, L.L.D., F.R.S., F.C.S., Hon. M.C.P.S., Corr. Mem. Roy. Acad. Sc. Turin, *Thorncliff, Old Trafford.*

1846, Jan. 27. Joynson, William. 41, *Fountain-street, Manchester.*

1843, Jan. 24. Kay, Samuel. 4, *Marsden-street, Manchester.*

1852, Jan. 17. Kennedy, John Lawson. 47, *Mosley-street, Manchester.*

1822, Apr. 26. Lane, Richard. *Chapel Walks, Half Moon-street, Manchester.*

1830, Apr. 30. Langton, William. *Manchester and Salford Bank, Mosley-street.*

1860, Jan. 24. Latham, Arthur George. *Messrs. Gladstone and Latham*, 24, *Cross-street.*

1850, Apr. 30. Leese, Joseph, junr. *Altrincham, Cheshire.*

DATE OF ELECTION.

1860, Jan. 24. Leigh, John, M.R.C.S. 26, *St. John's-street, Deansgate, Manchester.*

1839, Oct. 29. Lockett, Joseph. 3, *St. Peter's-square, Manchester.*

1857, Jan. 27. Longridge, R. B. 1, *New Brown-street, Market-street, Manchester.*

1842, Apr. 19. Love, Benjamin. *West Bank, Bowdon.*

1854, Jan. 24. Lowe, George Cliffe. 26, *St. Ann's-street.*

1850, Apr. 30. Lund, Edward. 22, *St. John's-street, Deansgate, Manchester.*

1855, Jan. 23. Lund, George T. 5, *Southgate, St. Mary's, Manchester.*

1859, Jan. 25. Lynde, James Gascoigne, M. Inst. C.E., F.G.S. *Town Hall, King-street, Manchester.*

1855, Oct. 30. Mabley, William Tudor. 14, *St. Ann's-square, Manchester.*

1859, Jan. 25. Maclure, John William, F.R.G.S. 2, *Bond-street, Cooper-street, Manchester.*

1829, Oct. 30. McConnell, James. *Union-street, Ancoats.*

1838, Apr. 17. McConnell, William. *Union-street, Ancoats.*

1844, Apr. 30. McDougall, Alexander. 11, *Riga-street, Hanover-street, Manchester.*

1823, Jan. 24. Macfarlane, John. *Ivy Lodge, Coney Hill, Bridge of Allan, Scotland.*

1849, Apr. 17. Manchester, The Right Rev. the Lord Bishop of, D.D., F.R.S., F.G.S., F.C.P.S., Corr. Mem. Arch. Inst. Rome. *Diocesan Registry, St. James's-square, Manchester.*

1858, Apr. 20. Mather, Colin. *Messrs. Mather and Platt's Iron Works, Salford.*

1842, Jan. 25. Mellor, Thomas. 204, *Oxford-road, Manchester.*

1837, Jan. 27. Mellor, William. *Lime Works, Ardwick.*

1859, Jan. 25. Molesworth, Rev. William Nassau, M.A. *Spotland, Rochdale.*

1849, Jan. 23. Morris, David. 1, *Market-place, near the Exchange, Manchester.*

DATE OF ELECTION.	
1859, Jan. 25.	Mosley, George. *Messrs. Mosley, Hurst and Co., St. Peter's-square.*
1848, Jan. 25.	Neild, Alfred. *Messrs. T. Hoyle and Sons, Mayfield Print Works.*
1822, Apr. 26.	Neild, William. *Messrs. T. Hoyle and Sons, Mayfield Print Works.*
1852, Jan. 27.	Nelson, James Emanuel. 27, *Piccadilly, Manchester.*
1854, Feb. 7.	Nevill, Thomas Henry. 17A, *George-street, Manchester.*
1860, Jan. 24.	Newall, Henry. *Hare Hill, Littleborough.*
1861, Jan. 22.	O'Neill, Charles. 9, *Grosvenor-street, Manchester.*
1844, Apr. 30.	Ormerod, Henry Mere. *Clarence Chambers,* 1, *Clarence-street, Manchester.*
1861, Apr. 30.	Parlane, James. *Messrs. Grey and Parlane,* 10, *Dickinson-street, Manchester.*
1861, Jan. 22.	Parr, George, junior. *Phœnix Works, Chapel-street, Ancoats.*
1833, Apr. 26.	Parry, John. *Messrs. Lockett and Co., St. Peter's-square, Manchester.*
1841, Apr. 20.	Peel, George, M. Inst. C. E. *Soho Foundry, Pollard-street, Ancoats.*
1861, Jan. 22.	Perring, John Shae, M. Inst. C. E. 104, *King-street, Manchester.*
1861. Jan. 22.	Pincoffs, Simon. 57, *George-street, Manchester.*
1857, Apr. 21.	Platt, William Wilkinson. *Messrs. Mather and Platt's Iron Works, Salford.*
1854, Jan. 24.	Pochin, Henry Davis. *Halliday, Pochin and Co., Quay-street, Salford.*
1860, Apr. 17.	Pocklington, Rev. Joseph N., B.A. 53, *Great Jackson-street, Hulme.*
1861, Jan. 22.	Preston, Francis. *Spindle and Fly Manufacturer, Ardwick.*

DATE OF ELECTION.

1861, Jan. 22. Radford, William. *Messrs. Cawley and Radford, 41, John Dalton-street.*

1854, Feb. 7. Ramsbottom John. *Crewe Station, London and North Western Railway Co.*

1859, Apr. 19. Ransome, Arthur, B.A. M.B. Cantab., M.R.C.S. 1, *St. Peter's-square, Manchester.*

1836, Apr. 29. Ransome, Joseph Atkinson, F.R.C.S. 1, *St. Peter's square, Manchester.*

1847, Jan. 26. Ransome, Thomas. *Princess-street, Manchester.*

1859, Jan. 25. Rideout, William J. *S. Crompton and Co.*, 11, *Church-street.*

1860, Jan. 24. Roberts, William, M.D. 10, *Chatham-street, Piccadilly.*

1822, Jan. 25. Robinson, Samuel. *Black Brook Cottage, Wilmslow.*

1858, Jan. 26. Roscoe, Henry Enfield, B.A., Ph.D., F.C.S., Professor of Chemistry, Owens College. *Owens College, Manchester.*

1842, Jan. 25. Royle, Alan. 28, *Ardwick Green, Manchester.*

1851, Apr. 29. Sandeman, Archibald, M.A., Professor of Mathematics, Owens College. *Owens College, Manchester.*

1857, Jan. 26. Satterthwaite, Michael, M.D. *Near Wilmslow.*

1842, Jan. 25. Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*

1858, Oct. 19. Sever, Charles. *Messrs. Cave and Sever, Palatine Buildings, Manchester.*

1855, Jan. 23. Sharp, Edmund Hamilton. *Seymour Grove, Old Trafford.*

1835, Oct. 30. Shuttleworth, John. *Wilton Polygon, Cheetham Hill.*

1852, Apr. 20. Sidebotham, Joseph. *Strines Printing Co.*, 19, *George-street.*

1859, Jan. 25. Slagg, John, jun. 12, *Pall Mall, Market-street, Manchester.*

DATE OF ELECTION.

1838, Jan. 26. Smith, George S. Fereday, M.A., F.G.S. 2, *Essex-street, King-street, Manchester.*

1845, Apr. 29. Smith, Robert Angus, Ph.D., F.R.S., F.C.S. 20, *Devonshire-street, All Saints, Manchester.*

1859, Jan. 25. Sowler, Thomas. *Courier Office,* 4, *St. Ann's-square, Manchester.*

1851, Apr. 29. Spence, Peter. *Alum Works, Miles Platting, Manchester.*

1852, Jan. 27. Standring, Thomas. 1, *Piccadilly.*

1834, Jan. 24. Stephens, Edward, M.D. 68, *Bridge-street, Manchester.*

1847, Apr. 20. Stephens, James. 68, *Bridge-street, Manchester.*

1858, Jan. 26. Stewart, Charles Patrick. *Atlas Works,* 88, *Great Bridgewater-street, Oxford-road.*

1814, Jan. 21. Stuart, Robert. *Ardwick Hall.*

1859, Jan. 25. Tait, Mortimer L. 95, *St. James's-street, Oxford-road.*

1856, Jan. 22. Taylor, John Edward. *Manchester Guardian Office, Cross-street.*

1860, Apr. 17. Trapp, Samuel Clement. 18, *Cooper-street, Manchester.*

1836, Apr. 29. Turner, James Aspinall, M.P. 50, *Cross-street, Manchester.*

1821, Apr. 19. Turner, Thomas, F.R.C.S. 67, *Mosley-street, Manchester.*

1860, Jan. 24. Unwin, William Cawthorne. *Messrs. Williamson Brothers, Engineers, Kendal.*

1861, Apr. 30. Vernon, George Venables, F.R.A.S. *Auburn-street, Piccadilly.*

1857, Jan. 27. Walker, Robert, M.D. 89, *Mosley-street, Manchester.*

1823, Jan. 24. Watkin, Absalom. 9, *Nicholas-street, Mosley-street, Manchester.*

DATE OF ELECTION.

1859, Jan. 25. Watson, John. *Rose Hill, Bowdon.*

1857, Jan. 27. Webb, Thomas George. *Glass Works, Kirby-street, Ancoats, Manchester.*

1858, Jan. 26. Whitehead, James, M.D. 87, *Mosley-street, Manchester.*

1839, Jan. 22. Whitworth, Joseph, F.R.S. *Chorlton-street, Portland-street, Manchester.*

1853, Apr. 19. Williamson, Samuel Walker. *St. Mark's-place, Cheetham Hill.*

1851, Apr. 29. Williamson, William Crawford, F.R.S., Professor of Natural History, Owens College. 172, *Oxford-road, Manchester,*

1859, Jan. 25. Wilde, Henry. 2, *Half Moon-street, St. Ann's-street, Manchester.*

1859, Apr. 19. Wilkinson, Thomas Read. *Manchester and Salford Bank, Mosley-street.*

1851, Jan. 21. Withington, George Bancroft. 24, *Brown-street, Manchester.*

1836, Jan. 22. Wood, William Rayner. *Singleton Lodge, near Manchester.*

1855, Oct. 30. Woodcock, Alonzo Buonaparte. *Orchard Bank, Altrincham.*

1860, Apr. 17. Woodcroft, Rufus Dewar. *Messrs. Roberts, Dale and Co., Cornbrook.*

1846, Apr. 21. Woodhead, George. *Old Hall, Mottram.*

1839, Apr. 30. Woods, Edward. 5, *Gloucester Crescent, Hyde Park, London.*

1860, Apr. 17. Woolley, George Stephen. 69, *Market-street, Manchester.*

1840, Apr 28. Worthington, Robert, F.R.A.S. 96, *King-street, Manchester.*

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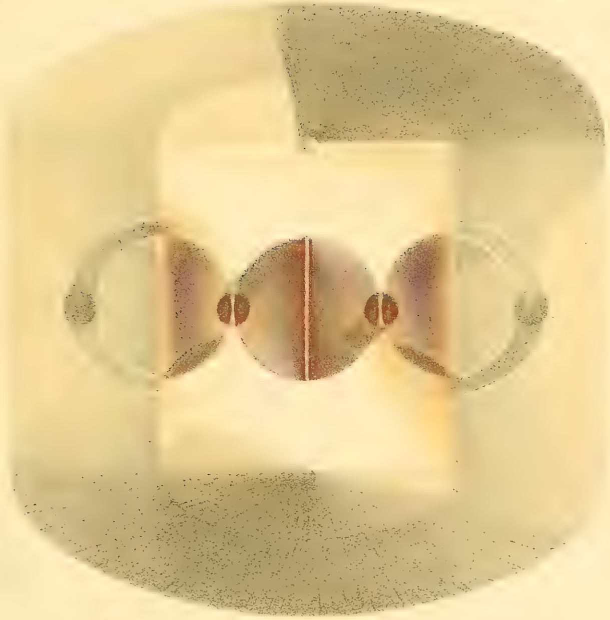


Vol. I. 3d Series. Literary & Philosophical Society of Manchester.

Reiffenstein & Rösch Lith[rs] Vienna.

Reiffenstein & Rösch Lithrs Vienna.

Fig. 2.

PLATE IV.

Fig. 1.

Fig. 2.

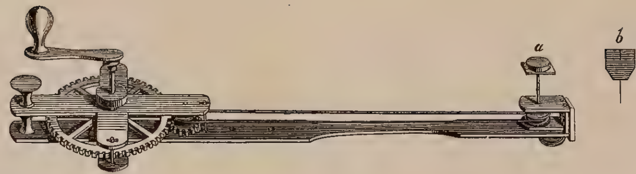


Fig. 3.

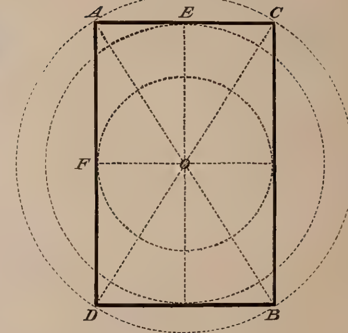


Fig. 4.

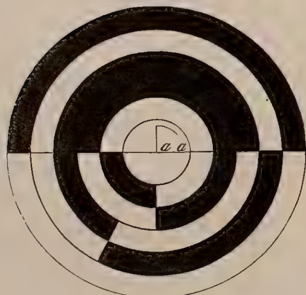


Fig. 5.

Fig. 8.

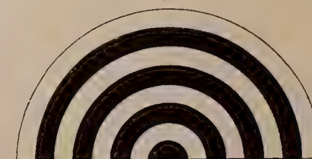


Fig. 6.

Fig. 7.

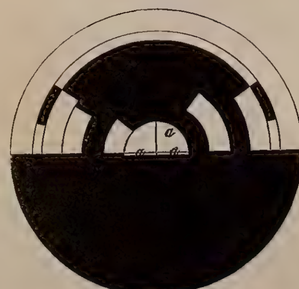


Fig. 9.

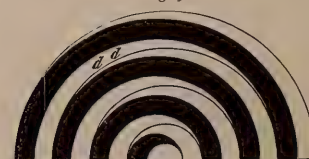


Fig. 6.

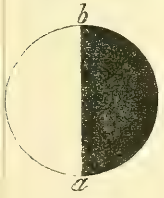
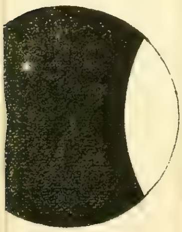


Fig. 10*a*

A



a

PLATE V.

Fig. 1.

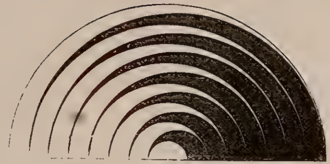


Fig. 2.

Fig. 3.

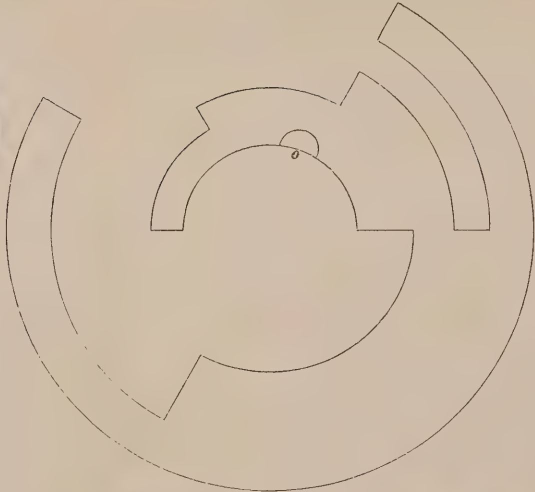


Fig. 4.

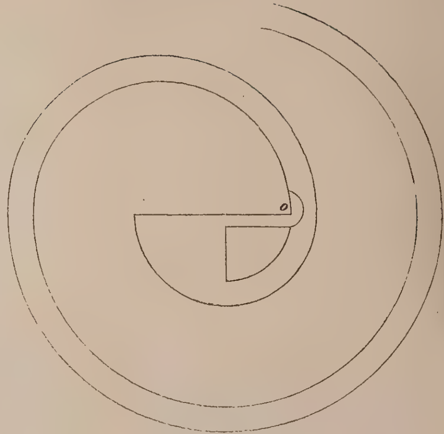


Fig. 5.

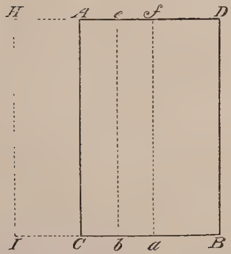


Fig. 6.

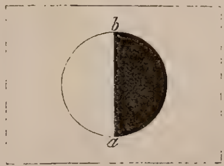


Fig. 7.

Fig. 8.

Fig. 9.

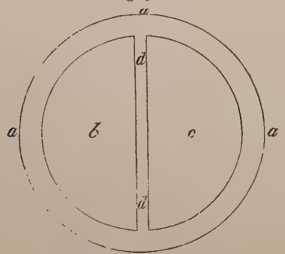


Fig. 10*a*

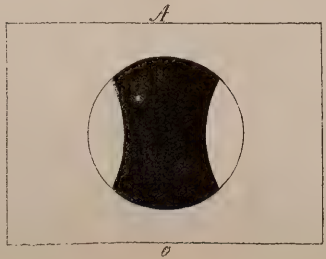


Fig. 10*b*

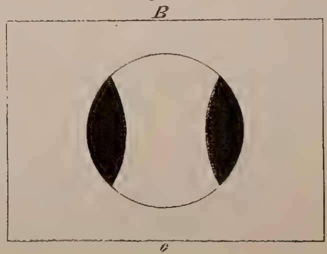
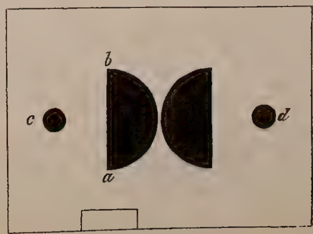
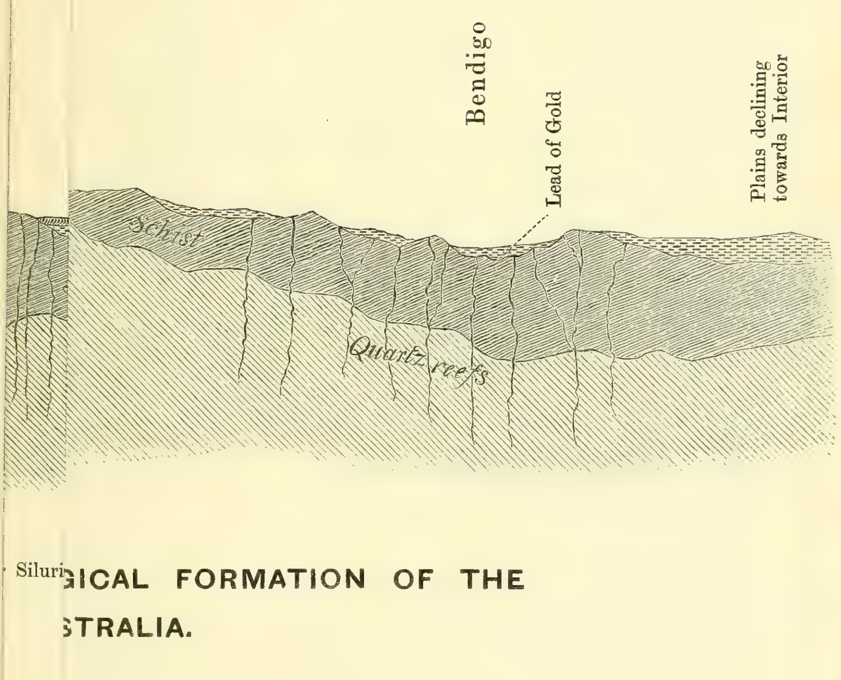


Fig. 11.



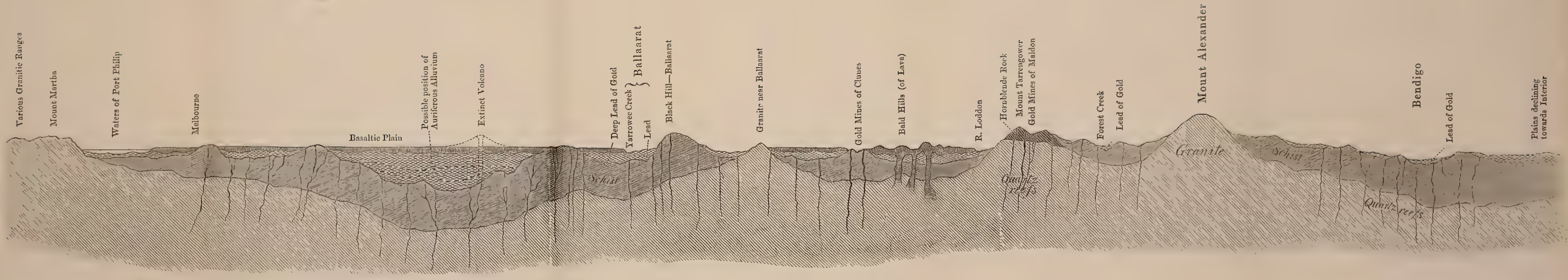


Siluri

GICAL FORMATION OF THE

STRALIA.

nying paper,
ntry.



Hornblende rock.

Basalt or lava.

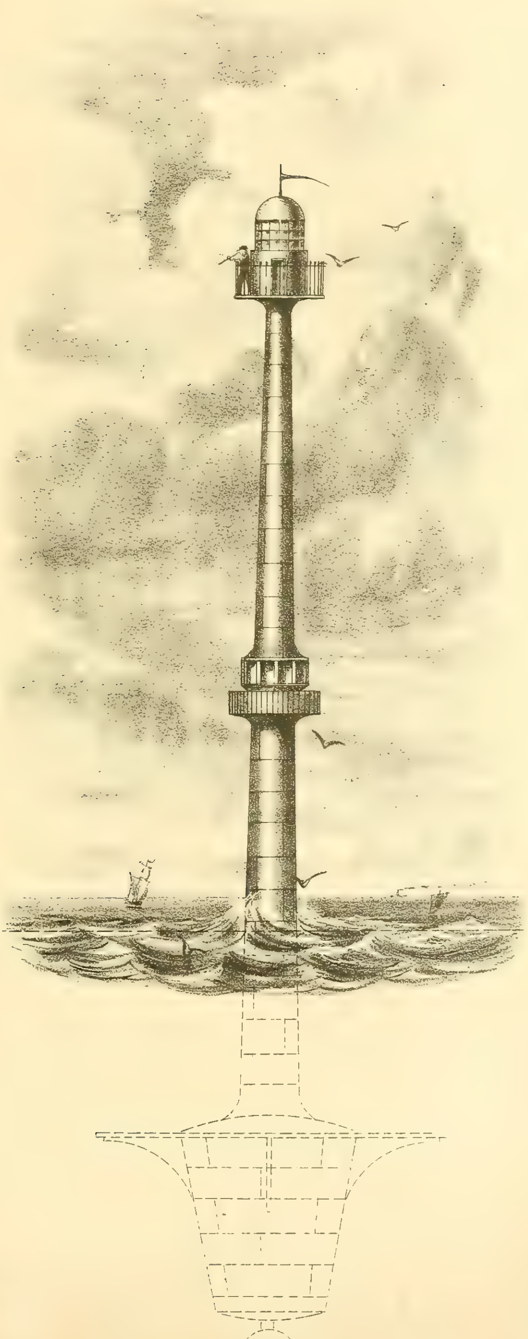
Alluvium or detritus, derived principally from schistose rocks. Auriferous in parts.

Slate or schist rock — of the Lower Silurian age — penetrated by quartz veins.

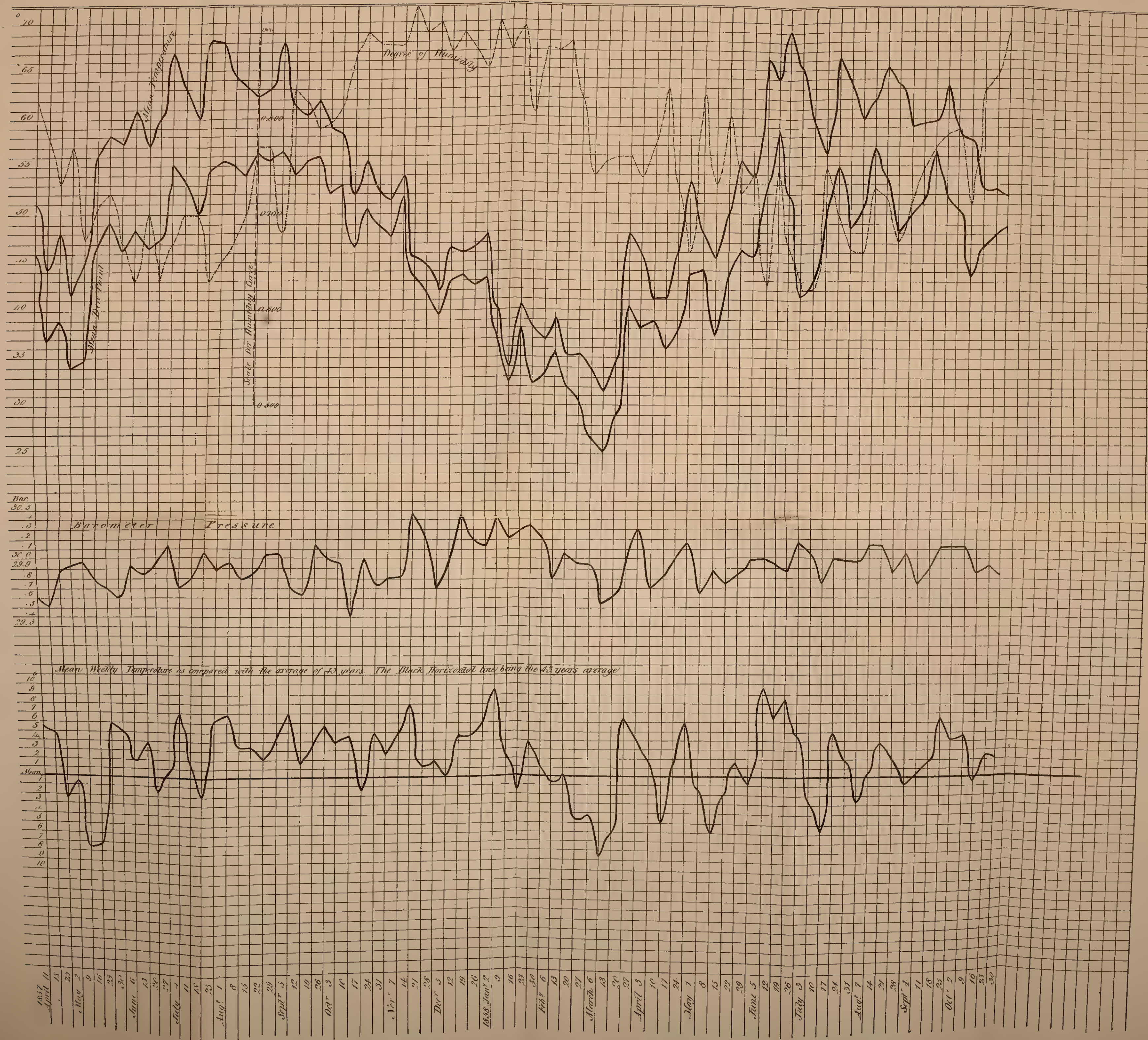
Granite.

SECTION ILLUSTRATIVE OF THE SUPPOSED GEOLOGICAL FORMATION OF THE GOLD FIELDS OF VICTORIA, AUSTRALIA.

N.B. This Section is merely explanatory of the accompanying paper, and is not a representation of any one line of country.

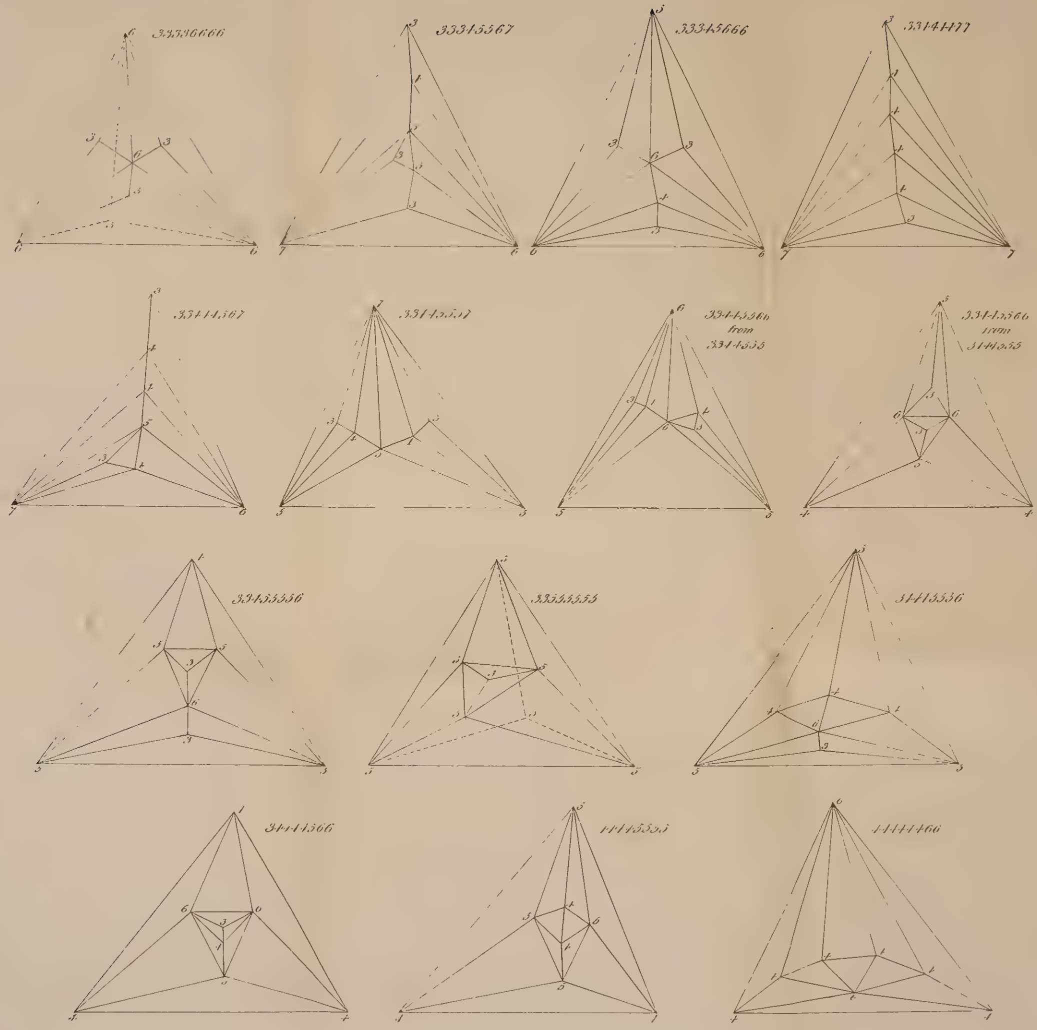


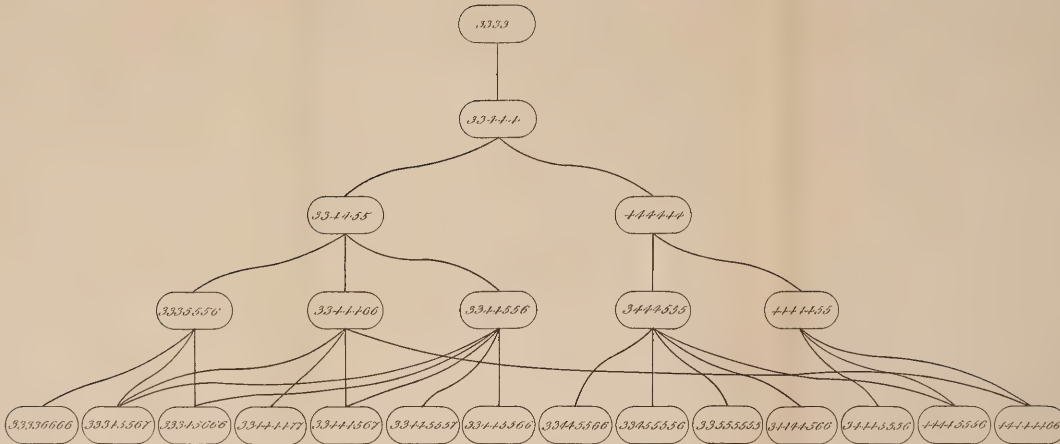

1857 Weeks ending
1858
Hooping Cough
Whooping Cough
Measles
Scarlatina
Scarlatina Scale
Pneumonia
Bronchitis and Catarrh
Continued Fever
Rheumatic Fever
Diarrhœa
Dysentery
Pleurisy



Mean Temperature
Mean Dew Point
Degree of Humidity
Scale for Humidity Curve
Barometer Pressure
Mean Weekly Temprature as compared with the average of 43 years. The Black Horizontal line being the 43 years average
1857 April
May
June
July
Augt
Septr
Octr
Novr
Decr
1858 Janry
Feby
March
April
May
June
July
Augt
Septr
Octr

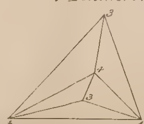
14 Octacrons





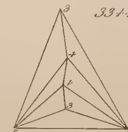
1 Tetracron 3333

1 Pentacron 33444

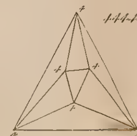


2 Hexacrons

334455

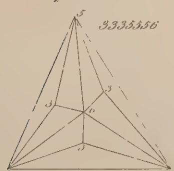


444444



5 Heptacrons

3335556



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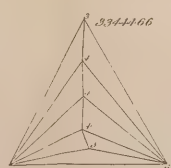

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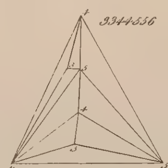

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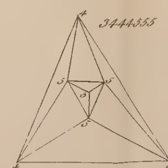

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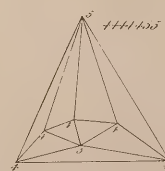
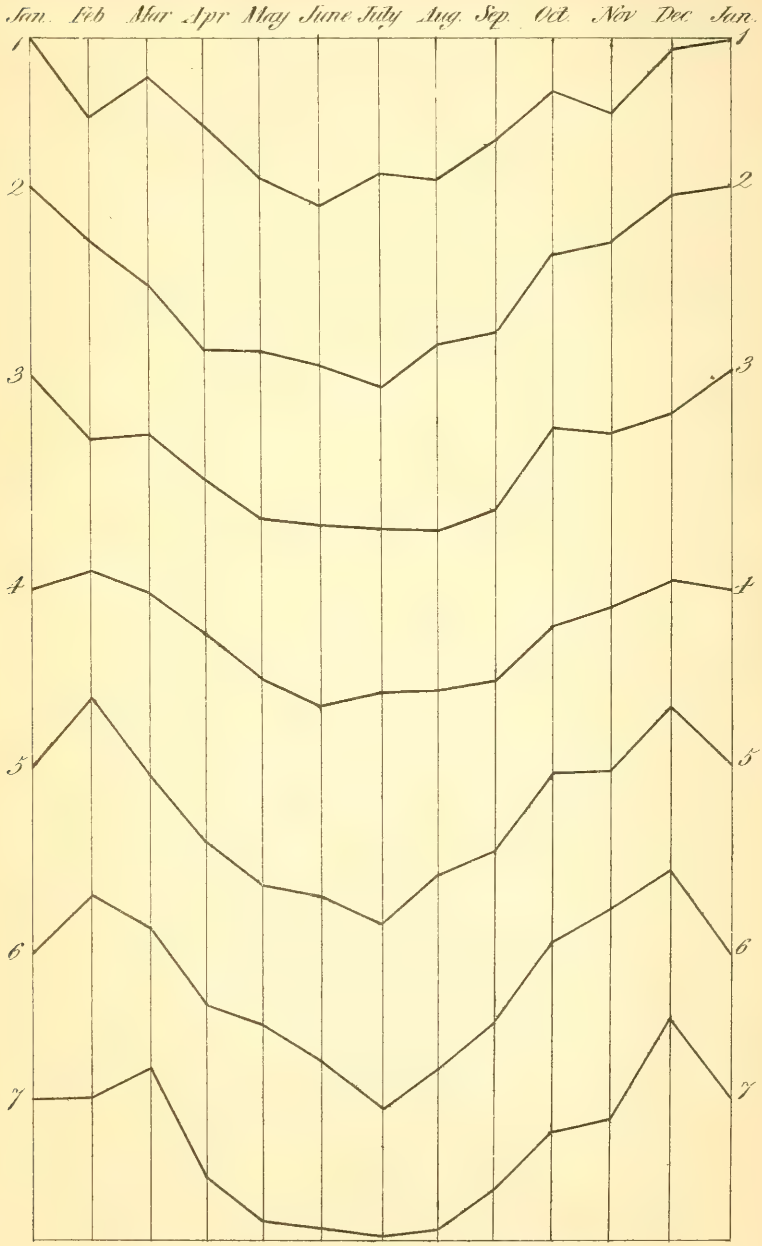
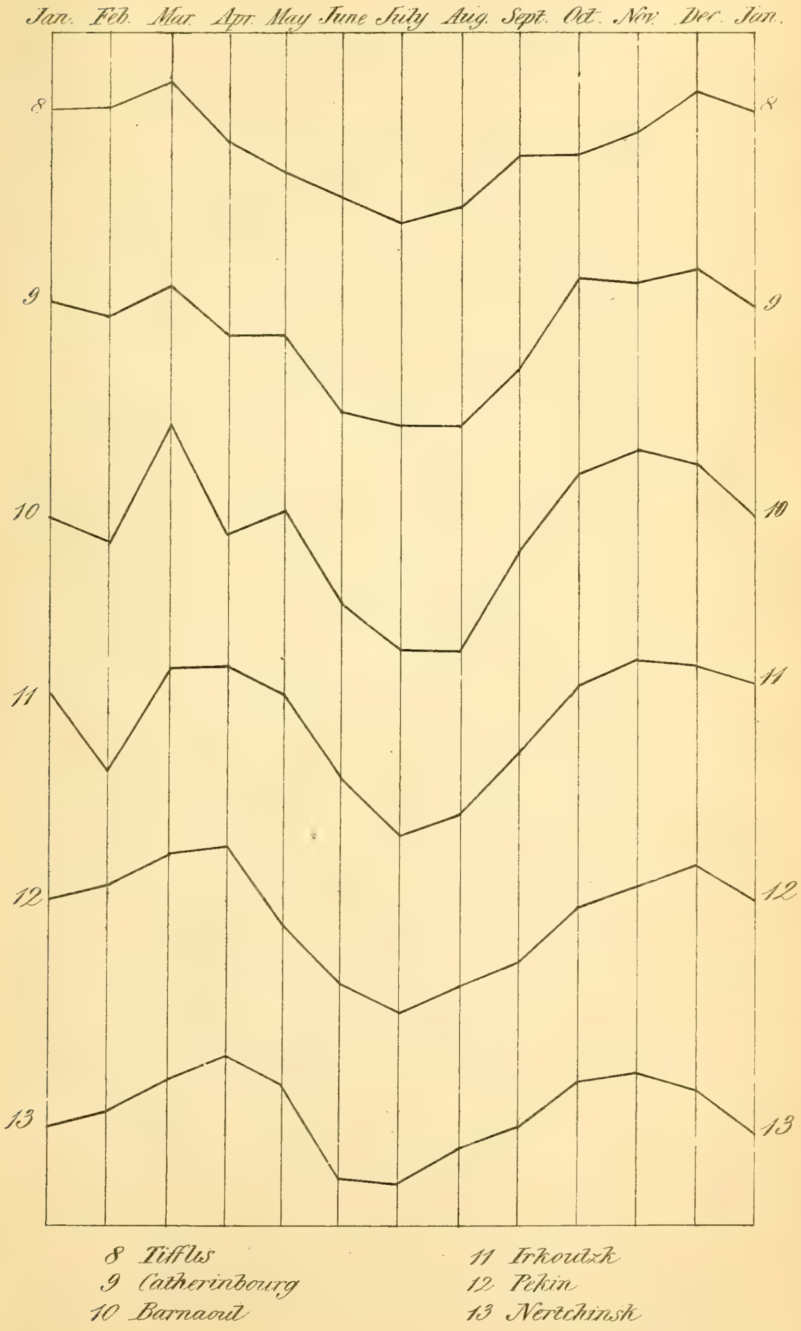


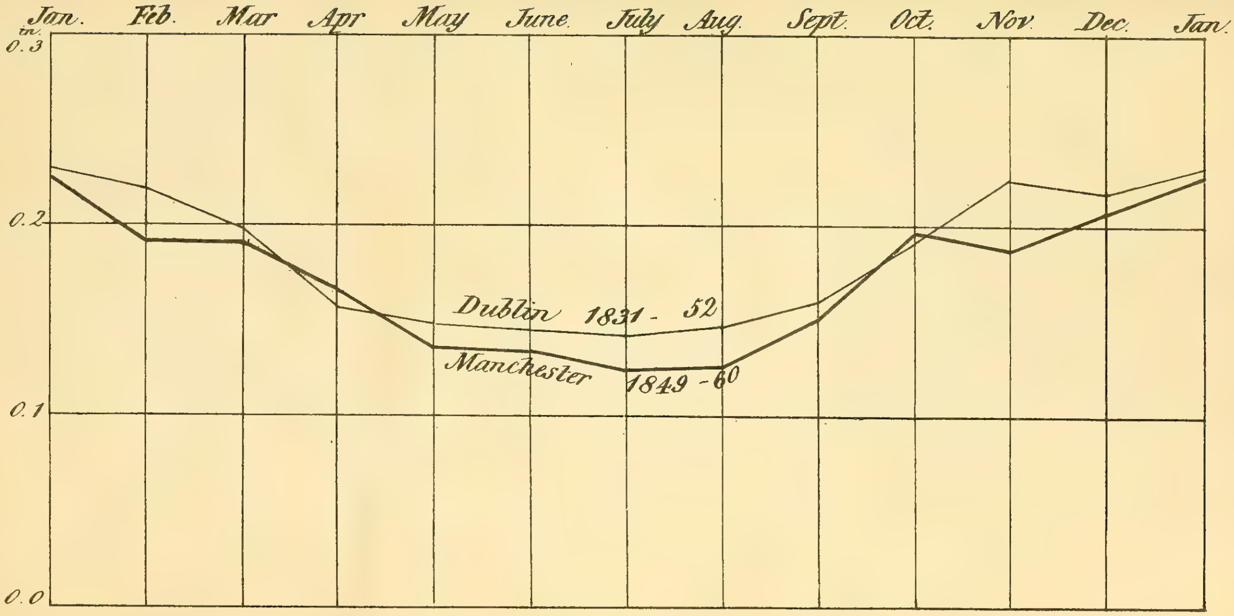
DISTURBANCES OF ATMOSPHERIC PRESSURE.



1. Dublin
2. Sandwick
3. Greenwich
4. Milan
5. Stockholm
6. St. Petersburgh
7. Lougan

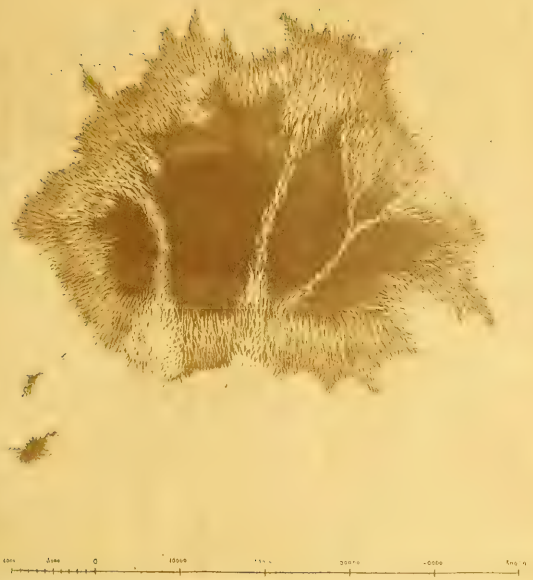
DISTURBANCES OF ATMOSPHERIC PRESSURE.





Monthly mean daily amount of the Irregular Oscillations of the Barometer at Manchester and Dublin.

1.



2

ILLUSTRATIONS OF MR. NASMYTH'S PAPER "ON THE STRUCTURE OF THE LUMINOUS ENVELOPE OF THE